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NOTES AND COMMENTS

The Behring Fur-Seals

There are few animals of greater biological interest than the fur-seals of the North Pacific, and all true naturalists are anxiously watching the negotiations now in progress between the British and American nations to prevent the complete sacrifice of these animals to the demands of commerce. At present they live in two great herds, frequenting respectively the Commander Islands off the Asiatic Coast, and the Pribylov Islands near Alaska. The herds have decreased in size during the last twenty-five years, and according to the Americans the decline is so ruinous that these animals have ceased to be of commercial value. For some years the Americans have been clamouring for more effective protection of the herds, and their demands led to the Paris arbitration, which resulted in the institution of a series of regulations to save further waste of seal life. This, it has been admitted, was merely a temporary expedient, and during the past two years a detailed study of the fur-seals on the Pribylov Islands has been carried on by an American and British Commission of naturalists, who have now come to a preliminary agreement as to the essential facts. The agreement has not yet been published in full, but it appears to show that the Americans have only too good reasons for believing that the seal herds require more efficient protection.

We are pleased to observe that the essential facts are summarised in an article in the December number of the *Contemporary Review*, in which the case from the naturalists' point of view is clearly and simply stated. It appears that the fur-seals live most of the year at sea, but are compelled to resort to the land for breeding purposes, as the young cannot swim for the first month. The animals, therefore, land on the islands of the Behring Sea. There they sort themselves into three classes—the breeding bulls or sea-catchie; the young bulls who have no chance of mating and live apart from the

main herd; and, thirdly, the females, all of whom of two years old and upwards are eligible for breeding. The old males land on the islands first and take up stations along the shore; when the females arrive they are immediately seized by the males, each of whom, by force or blandishments, collects a number of females into a harem of, on an average, about eighteen individuals. During the summer all the females are fertilised, and owing to the peculiar bilobed structure of the uterus each female can give birth to a pup every year, the opposite sides being used in alternate years. Hence, as the period of gestation is 355 days, it follows that, except for ten days in the summer, every female seal above two years old is always pregnant. Moreover, owing to the extraordinary jealousy of these animals, no mother-seal will give suck to any pup except her own. Hence, if a mother be killed between the birth of her pup and the end of October or beginning of November, when the young are weaned, her pup is inevitably starved to death. Thus every female seal killed between June and October, both months inclusive, means the death of three seals—a mother, a pup, and a foetus. At any other time of the year every female killed means the loss of two lives. At present there is a close time for seals in the Eastern Behring Sea for May, June and July. A healthy pup takes ten days in which to starve to death, and it is, therefore, very significant that after the middle of August vast numbers of young pups are found on the Pribylov Islands starved to death. It is admitted by both sides that many, possibly a great majority, of the deaths of these pups are due to the killing of the mothers at sea. Hence the Americans say that the close time ought to be extended at least until the end of October, a decision which would practically mean the end of pelagic sealing.

It may be retorted that if the Canadians are to be debarred from killing fur-seals at sea the Americans ought to be prevented from killing them on shore. But the conditions are totally different. On shore only non-breeding males with perfect skins are killed. No females or breeding males are taken. But at sea no such selection is possible; the sex cannot be determined until the seal is killed. Many of the seals escape with fatal wounds, and as the females are less active than the males and are often hampered by the presence of their young, they are more easily captured. Hence the majority of the seals killed at sea are females. The economic value of the pelagic seal industry is now insignificant, and as it appears to be admitted by both sides that the herds of fur-seals are being greatly reduced in numbers by the excessive killing at sea of female seals and the consequent starvation of nearly 20 per cent. of the young, it is to be hoped that effective measures may be taken to prevent this inhuman and wasteful slaughter.

The Lepidosiren of South America

At the last meeting of the Cambridge Philosophical Society on December 6, Mr J. Graham Kerr gave a short account of his recent expedition to the interior of the Gran Chaco of Paraguay for the purpose of investigating the habits and development of *Lepidosiren paradoxa*. It will be remembered that Mr Kerr was aided by a grant from the Balfour fund, and was accompanied by Mr J. S. Budgett of Trinity College.

Lepidosiren occurs in considerable quantity in the swamps towards the centre of the Gran Chaco boreal. It is sluggish in habits, wriggling slowly about among the thick vegetation of the swamp. At short but very irregular intervals it visits the surface and takes a breath of air. Its food consists mainly of large Ampullarias and masses of confervoid algae. The young are to a greater extent vegetable feeders than are the adults. Lepidosiren makes a burrow in the ground at the bottom of the swamp and lines it with soft grass. In this the eggs are laid. The papillae on the hind limb of the male grow out into long filaments during the breeding season, and during life these are blood-red in colour. They appear to be ornamental structures.

The eggs are very large—about 7mm. in diameter. Coelomic eggs have a thick gelatinous coat: in fertilised and developing eggs this becomes thin and horny. Segmentation is during its later stages holoblastic and unequal. Gastrulation takes place in a manner which recalls that of Urodele Amphibia, and of Cyclostomes. Eventually a tadpole larva is hatched out. This develops large external gills and a very large sucker of the Amphibian type.

The external gills and sucker disappear about six weeks after hatching. At the same time the colour of the young Lepidosiren becomes much darker and the animals become much more lively in their habits. For the first ten to twelve weeks of their free existence the young do not eat but live on the yolk in the walls of their intestine.

A remarkable habit of Lepidosiren was observed, namely, that their normally very dark colour becomes nearly white during the night. The black chromatophores shrink up during the hours of darkness—large yellow chromatophores, which are also present, remaining expanded.

During the dry season the Lepidosiren retreats into the mud in which it remains breathing by means of an air-hole until the waters return and set it free.

Matters Molluscan

The fourth and last note by Mr Félix Bernard on the development and morphology of the hinge in bivalve Mollusca has just been

received (*Bull. Soc. Géol. France*, sér. iii., tom. xxv., pp. 559-66, figs.). In the third note (vide *Nat. Sci.*, vol. x., p. 88), he dealt with the Anisomyaria, in the present one he deals with certain of the Heterodonta which had been passed by in his first note. The Unionidae, however, still remain to be studied. The author finds that the Heterodonta naturally fall into two groups which he provisionally, and without at present attributing any taxonomic value to them, calls the 'Lucinoid' and the 'Cyrenoid.' To the former he now refers the Cardiidae (cockles), Donacidae, Corbulidae, the Tellinacea, the Pholadidae, Trigoniidae, and Dreyssensiidae.

This decision as regards the last-named family is very important, in that it confirms the conclusions of competent anatomists that it was not, as always previously maintained, allied to the Mytilidae. The Chamacea and Rudistes prove a highly interesting group with respect to their development, for the hinge of the normal *Chama* is in its early stages Lucinoid; in the 'reversed' individuals, on the other hand, and in the Diceratidae, it is Cyrenoid, whilst in the Rudistes there is great diversity.

We still await with interest the yet promised monograph on the whole subject. Meantime, those who care for the study will find some very important theoretical suggestions in Mr Bernard's paper on *Conchylocardia* (*Journ. de Conchyl.*, July 1896 [1897], 1 pl., figs.).

These minute little bivalves, the largest of which only attains in its maximum diameter 1.3 mm., constitute a new type of pelecypod, and in their hinge present an early stage of the development of that in the Carditidae, Astartidae and Crassatellidae, especially the first-named. Not that they are to be regarded as an ancestral form; on the contrary, Mr Bernard considers the case to be one of neoteny (or paedogenesis). The genus would be derived from a normal type of *Cardita*, in which reproduction began early and which would thus be arrested in its growth. A similar condition of affairs is manifest in *Philobrya*, which genus forms the subject of another paper (*Journ. de Conchyl.*, Jan. 1897, 1 pl., figs.). *Hochstetteria*, if not generically identical with this, is at least very nearly allied indeed, and both present close affinities with the Aviculidae. Dall has stated very positively (so much so that we have quoted it as a fact—vol. xi., p. 341) that *Philobrya* passes through a glochidial stage; but it appears that this was an ingenious but unverified hypothesis based on an apparent similarity in shape between the young shells of this genus and of the Unionidae. Yet another paper by Mr Bernard (*Ann. Sci. Nat. Zool.*, sér. viii., tom. iv., pp. 221-52, 2 pls., figs.) describes the detailed anatomy of *Chlamydoconcha orcutti*, Dall, which might be described as a marine bivalve slug, for the two valves of the shell are reduced

to two long narrow plates, and entirely bedded, opened out side by side, in folds of the mantle. All trace of an adductor muscle is wanting. Otherwise, in its internal anatomy, *Chlamydoconcha* closely resembles *Ephippodonta*, described by Martin F. Woodward, in *Proc. Malac. Soc.*, vol. i., pp. 20-26.

A Comprehensive Work on Moths

The British Museum (Natural History) has entered on a gigantic undertaking. Although publicity is especially desired, the authorities apparently have not considered it necessary to send their circular to the general scientific press. We accordingly reprint the circular issued by Sir William Flower from the *Entomologists' Monthly Magazine* for November:—

"Prospectus of a series of Volumes on the Lepidoptera Phalaenae of the Whole World, to be published by the Trustees of the British Museum.

"The Trustees of the British Museum having sanctioned the publication of a series of volumes on the Moths of the World, and entrusted Sir George Hampson with the commencement of the work, I beg to call your attention, as being interested in the classification of the Lepidoptera, to the following scheme, which has been approved of for the work. The chief want of entomologists working at this subject at the present time is a comparative analysis of the genera in each family, on the lines of the classification originated by Herrich-Schäffer and Lederere for the European fauna, and amplified of late years by Snellen, Meyrick, J. B. Smith, E. L. Ragonot and others, for various groups of the Palaearctic, Nearctic, Oriental and Australian faunas, and it is thought that the best way to supply this want is by the publication of such a work as the following:—

"(1) The size of the work to be large 8vo, similar to the Catalogue of Birds, Reptiles, &c., each volume to consist of about 500 pages, the exact number being regulated with a view to completing the classification of a family or subfamily; each volume to be complete in itself, with its own index.

"(2) The general arrangement of the work, and of each family and genus, to be from the more specialised to the most generalised forms, the arrangements being modified so as to make the system as natural as possible.

"(3) The work to contain synopses and descriptions of the families, genera and species of Moths, every described species, about which any exact information can be gained, being included, references only being given to those whose systematic position cannot be ascertained with tolerable ex-

actitude, and no new species being described except such as are represented in the British National Collection.

"(4) No catalogue of specimens will be included, but a somewhat full and minute account of the range of the species, references to all 'types,' to collectors' names when known, and to series of specimens of which an account is given in published works.

"(5) The 'type' of each genus will be indicated by being placed after the reference to the genus, whether it be retained, treated as a sub-genus or synonym.

"(6) The references to the species will be confined to the original reference, to one for each synonym, with dates of publication, to the best figure of the species, to the best description and figure of the early stages, and to such faunistic works and catalogues as are of most general use and importance.

"(7) Local races will be treated as sub-species under sub-headings, with their own references and synonymy.

"(8) Each genus, and all the more prominent sections of genera, will be illustrated by process blocks in the text, showing the facies of a typical species of the genus or section, and all the more prominent details of structure on which the genus is founded.

"(9) An atlas of coloured 8vo plates will also be issued, giving half-figures of as many as possible of the species which have never before been satisfactorily figured, especially of 'types' in the British Museum. This will be issued in parts, as is convenient, and will be sold separately from the volumes; it will contain no letterpress except the explanation of the plates, giving the names of the species figured, reference to the pages of the volume in which the descriptions will be found, and the country where the species will be found.

"(10) The order of the families will, in the main, follow that adopted by Mr E. Meyrick in his recent work on British Lepidoptera, and, commencing with the Syntomidae, will work down through the Arctiadae and Agaristidae to the Noctuidae and Eymantriadae; then, beginning again with the Saturniadae and their allies, will work downwards by the Sphingidae to the Notodontidae, Geometridae and Uraniadae; then by the Lasiocampidae, Limacodidae, Cossidae, Psychidae and Castniadae to the Zygaenidae; then by the Drepanidae and Thyrididae to the Pyralidae, Sesiadae and Tineidae; and ending with the Hepialidae and Micropterygidae.

"It is obvious that complete success in carrying out the above scheme will depend on the willingness of entomologists and museums to send specimens of described species which are not available in London, and the authorities of the British Museum (Natural History) hope that as much help in this way, as possible, may be given when application is made for the loan of specimens, which, when examined and figured, will be carefully packed and returned, the Museum paying the carriage going and coming."

(Signed) "W. H. FLOWER, Director."

The chief points to be noted in the above scheme are the publication of both text and plates in octavo, a much more handy form than quarto; the promise to figure unfigured forms, a promise of inestimable value; and the inclusion of a proper index in each volume. We trust the bait held out to foreigners, the figuring of their unfigured species, will induce those who wish to further the science of entomology to heap coals of fire on an institution which cannot itself (and in our opinion quite properly cannot) allow types to leave its walls.

A PARASITE ON COCKROACH EGGS

MR E. BORDAGE, director of the Natural History Museum of the island of Réunion, has been able to throw some light on the life-history of the obscure hymenopterous genus *Evania* (*C. R. Acad. Sci.*, 1896). The insects of this genus were known to pass the larval stage parasitically at the expense of cockroaches. By breeding a specimen of *E. desjardinsii* from a cockroach's egg-capsule, Mr Bordage has confirmed the opinion that the evaniid grub feeds on the eggs, and not in the body of its host. He has observed a female *Evania* pursuing a female *Periplaneta americana* in the act of extruding her egg-capsule. As this *Evania* is presumably a Mascarene insect, while the cockroach is an introduced American species, it appears that the parasitic grub can be reared in any blattid egg-capsule which affords it enough food and room, and that each species of *Evania* is not confined to a particular cockroach as its host.

LEG-CASTING AMONG STICK-INSECTS

SOME interesting observations on the limb-casting of two species of Stick-insect (Phasmidae) are also due to Mr Bordage (*C. R. Acad. Sci.*, 1897). These insects occasionally threw off a leg that had been seized or bitten by an ant, or held or irritated by the experimenter. It was found that larvae parted with their limbs much more readily than adults. The insects have the power of replacing the lost leg, but, as might be expected, the newly-formed limb is shorter than its fellow which has not been thrown off. An interesting point is, that the foot of a regenerated leg has only four tarsal

segments instead of the normal number, five. Mr Bordage mentions the observations of Messrs Bateson and Brindley on variation in the number of tarsal segments in cockroaches' feet. They regarded five as the primitive number of segments; Mr Bordage, on the other hand, believes that the appearance of a four-segmented foot in a species with normally five segments, must be considered as a case of atavism. We are inclined to think that the presence of the five-segmented foot in so many orders of insects raises a strong presumption that it is a primitive character, and that a reduction in the number of segments has been acquired in certain families; especially as the reduction is often most marked in insects which are most highly modified for aquatic life.

Swedish Arctic Expedition

In addition to the information which we were able to furnish in our last number, we now learn that Professor Nathorst has secured a vessel, namely, the *Antarctic*, in which, it may be remembered, Mr Borchgrevink accompanied Captain Kristensen and Mr Bull to the south polar regions in 1893-95. The measurement of an arc of the meridian on the east coast of Spitzbergen will, it is hoped, be undertaken by another expedition, specially equipped for that purpose, and will be the task of more than one year. The Swedish organiser of this work is the astronomer Professor Jäderin, and it is hoped to obtain the co-operation of Russia.

Apropos of Mr Borchgrevink, who is being financed by Sir George Newnes, we note that he has bought the s.s. *Pollux* for his forthcoming Antarctic expedition.

German Deep Sea Expedition

We learn from the London *Daily Chronicle* that a parliamentary paper has been circulated among the members of the Reichstag with reference to an item of Mk.300,000 (£15,000) which will appear in the Estimates in support of a German deep sea expedition. This document passes in review the successful efforts made by other maritime nations in this direction, and notably by Great Britain, with the remarkable 'Challenger' expedition. Germany, it is pointed out, is the only European nation with any pretensions to maritime power which has taken little or no part in revealing the secrets of the deep seas, and it behoves her now to make amends for her past negligence, especially when the work to be done is still so vast and so varied. Accordingly, it is proposed that a suitable steamer shall be chartered and leave Germany next August. Her course would be first between the north of Scotland and the Shetland Islands, and then south to the Canaries and Cape Verde, along the West African coast to the Cape, and to the currents of the Antarctic. Special

investigations will be made in that region where the cold currents of the Antarctic and the warm Indian currents meet. The expedition will then cruise through the Indian Ocean and the Red Sea, reaching home after an absence which is estimated at nine months.

We are able to add that the leader will be Professor Chun, director of the Zoological Institute of Breslau. The success of the expedition, if started, will therefore be assured.

The Cricket as Thermometer

The *American Naturalist*, despite its change of editors, shows no falling off in vigour. The November number contains a novel observation by Mr A. E. Dolbear, and since his note is only a little one, we venture to steal it whole:—

"An individual cricket chirps with no great regularity when by himself and the chirping is intermittent, especially in the daytime. At night when great numbers are chirping the regularity is astonishing, for one may hear all the crickets in a field chirping synchronously, keeping time as if led by the wand of a conductor. When the numbers are so great, the resting spells of individuals are unnoticed, but when the latter recommence they not only assume the same rate but the same beat as the rest in that field. The crickets in an adjoining field will have the same rate, that is, will make the same number of chirps per minute, but with a different beat, as one may easily perceive by listening.

"The rate of chirp seems to be entirely determined by the temperature, and this to such a degree that one may easily compute the temperature when the number of chirps per minute is known.

"Thus at 60° F. the rate is 80 per minute.

"At 70° F. the rate is 120 a minute, a change of four chirps a minute for each change of one degree. Below a temperature of 50° the cricket has no energy to waste in music and there would be but 40 chirps per minute.

"One may express this relation between temperature and chirp rate thus:

"Let T stand for temperature, and N the rate per minute.

$$T = 50 + \frac{N - 40}{4}$$

"For example. What is the temperature when the concert of crickets is 100 per minute?

$$T = 50 + \frac{100 - 4}{4} = 65°."$$

This is an observation suggestive of further inquiry. First, we should like to know whether it applies to all kinds of chirping or stridulating insects. And incidentally we may ask—what is the species, or even the genus, referred to by Mr Dolbear? There are

are least six species, belonging to two genera, of what are ordinarily called crickets in North America. Those good people who ask us to use plain English names, instead of crackjaw Graeco-Latin compounds, would, we imagine, be hard put to it to decide what Mr Dolbear means by a cricket. The only sure thing is that he does not mean the 'cricket on the hearth,' which is what we usually understand by the cricket in England. Further, it would be of interest to learn whether this regular relation to temperature is not affected by moisture in the air, by altitude or barometric pressure, by locality generally, or by other varying conditions of existence. Until these questions are answered it is not likely that any species of cricket will prove a serious rival to Fahrenheit, Celsius, or Réaumur.

Dr William Francis and the "Annals"

The December number of our venerable contemporary, *The Annals and Magazine of Natural History*, completes volume xx., the last volume of the sixth series. This is the end of the 120th volume, and the 60th year of the *Annals*' existence, the first number having been published in January 1838 under the title "Annals of Natural History; or Magazine of Zoology, Botany, and Geology (being a continuation of the 'Magazine of Zoology and Botany,' and Sir W. J. Hooker's 'Botanical Companion'), conducted by Sir W. Jardine, Bart., P. J. Selby, Esq., Dr Johnston, Sir W. J. Hooker, Regius Professor of Botany, and Richard Taylor, F.L.S. London: Printed and published by R. & J. E. Taylor." The *Annals* is, therefore, the oldest magazine devoted to the natural sciences existing in this country, and we, as one of the youngest, offer to it our homage and congratulations on this its Diamond Jubilee.

Others who have assisted in the editorship of the *Annals* have been David Don (1840), C. C. Babington (1842), J. H. Balfour (1842), J. E. Gray (1858), A. Henfrey (1858), William Francis (1859), W. S. Dallas (1868), A. C. L. G. Günther (1875), W. Carruthers (1878). Although the name of Dr Francis does not appear on the title page until 1859, since when he has been the really responsible editor, we learn from the preface to the second series that he was intimately connected with the editorship from the beginning. We doubt if there is anyone who can show a longer record of unostentatious, unremitting work in the cause of science. It is therefore not surprising to learn from an announcement in this December number, that Dr Francis, to use his own language, avails himself of this occasion to pass the responsible editorship over to his son, who, for some years past, has largely assisted him in the management, and who is fortunate in retaining the co-operation of the two co-editors whose names appear on the title page. The good wishes of all working naturalists will follow Dr Francis into his

retirement, and accompany his son on the future course of the *Annals*.

Bulletin of the Liverpool Museums

Dr H. O. Forbes, the director of the Derby and Mayer Museums at Liverpool, is certainly to be congratulated on the form in which his publication, entitled *Bulletin of the Liverpool Museums under the City Council*, makes its first appearance. As for the appearance itself, we need not repeat, after so short an interval, the remarks that we recently felt urged to make as to the multiplication of scientific periodicals. The object of the present publication is to make known the results of the investigations carried on in the laboratories attached to the museums, and to record the observations made on the animals living in the Aquarium. It is to be issued quarterly, at an approximate price of 8s. per annum. The price of the first number, stated on the wrapper (as we are glad to see) to have been issued on August 4, is 2s. 6d. It leads off with an account of the Derby and Mayer Museums, with portraits of their founders, the thirteenth Earl of Derby and Mr Joseph Mayer, also of Sir William Brown, who provided the building for the public library. The bulk of the number, eighteen pages, is made up of the first instalment of a "Catalogue of the Parrots (Psittaci) in the Derby Museum," by Henry O. Forbes and Herbert C. Robinson; this is illustrated by two coloured plates drawn by Mr J. Smit. We understand that this catalogue will eventually be issued as an independent volume. The "Notes from the Museums and the Aquarium," which conclude the number, contain: an account of a parasitic copepod, *Penella blainvillei*, attached to a flying fish, and itself covered with a colony of small cirripedes, *Conchoderma virgata*; the history of an ordinary stickleback living in salt water; a description of the successful feeding of young trout on powdered sheep's liver; notes on *Malapterurus electricus*; remarks on recent acquisitions by the Mayer Museum, viz., Cypriote antiquities, Neolithic flint implements from Egypt, West African masks. The last note of all being both short and interesting, we quote it in full as a sample of the rest.

'Medicine' at the Liverpool Museums

"It may not be without interest, from a folk-lore point of view, to place on record that, but a few days ago, an Irish lad suffering badly from scrofulous sores was brought to the Mayer Museum by his parents, who earnestly besought the authorities that they might be allowed to touch the child's neck with an Irish Stone Celt, exhibited in one of the cases. It was unavailing to try and persuade the deluded and superstitious couple that no possible good could follow such an application. As their faith in the efficacy of the Stone could not be shaken, and they were loth to go away without

being allowed to try this, in their belief, unfailing remedy, opposition was finally, and not without some hesitation, withdrawn, and the ancient implement placed in their hands. After the operation the parents departed happy, grateful, and in the most perfect confidence that their child would be healed, and not without expressions of surprise that so great a boon had been conferred on them without the fee which they were prepared, and that very gladly, to pay."

Geology in Japan

A general account of the Geological Survey of Japan was given in *Natural Science* for February 1894 (vol. iv., pp. 105-111). We are glad to note that the cartographical labours of this survey are making good progress. A map of Japan proper, in five divisions, on a scale of 1 : 100,000 is already completed, and special maps on a scale of 1 : 200,000 have been published for about half the same area. These are accompanied by explanations, written in Japanese. Besides this, numerous detailed surveys on a larger scale are constantly being carried out to meet various official and private requests. Often these are in connection with earthquakes and eruptions, with which geological phenomena Japan is so favoured, and which often seriously affect work in the mining districts. An examination of the water-bearing strata in the Yokohama, Chiba, and Gumma prefectures has recently been concluded. Preliminary and special surveys have been made in the northern island, Hokkaidō. In connection with the war with China, a special geological and agronomical reconnaissance was made of the Liao-tung peninsula. As a result of that war the geological survey of the empire will soon be carried into Formosa. On this island a few fragmentary notes, stated by the Japanese authorities to be very imperfect, have been published by Gordon, Von Richthofen, Guppy, Kleinwächter, and others. Reports on the coal and gold found in the island have been issued by Sotokufu; and the geography of the Taiwan islands, written by Mr Ogawa, contains some references to their geology and agronomy. Lately, Dr Kotō, professor of geology at Tokyō University, and his assistant, Mr N. Yamasaki, have travelled round the main island.

There is in Japan no separate body like our Ordnance Survey, but the Geological Survey has to make its own topographical maps. All the work to which we have here alluded is carried on by a staff of six geologists, four topographers, and eight cartographers.

A New Manure

The above account does not exhaust the work of the Geological Survey of Japan, for it says nothing of the eight chemists and seven agronomists. The agronomical section treads on the heels of the

geologists, studying the relations of the soils to the underlying rocks and investigating their capabilities.

In 1893 this section made a very important discovery, which it has since been carrying to a conclusion. A fruitless search had long been conducted for deposits of mineral fertilisers, but in that year a phosphate deposit was found in marine beds supposed to be of Miocene age, in Hyūga province in Kyūshū, extending over an area about 40 kilometers long by 6 wide. Within this area phosphatic marl occurs sporadically in nodular layers, chiefly along the sea-coast. The nodules contain some 20 per cent. of phosphoric acid, and a very small quantity of calcium carbonate, so that the extraction of the acid is rendered more easy. An attempt is being made to produce a super-phosphate by extracting the acid and pouring the liquor on the powdered phosphate. The process has not yet reached such perfection as to be a commercial success; but experiments with pot-plants have shown that the fertilising powers of the new phosphate are considerable. The origin of this peculiar deposit is not yet understood: it certainly is not derived from fossil vertebrates.

One is always hearing about the depressed state of agriculture in Great Britain, and we do not suppose that the mere existence of a corps of agronomists would prove an all-sufficient remedy. But when we see other countries, Germany, Russia, America, Japan, and the rest, making exhaustive studies of their soils, and ever searching out new tracts capable of cultivation, or new substances of value to the farmer, then we ask—Where is our Agronomical Department?

Geology in Queensland

From the *Annual Progress Report* of the Queensland Geological Survey for 1895, just to hand, we learn that Mr Sydney B. J. Skertchly was appointed Assistant Geologist on 29th June 1895. Since then Mr Skertchly has been examining the Deep Lead of tin at Herberton, the tin and copper mines in the Watsonville, Montalbion, and Chillagoe districts, the gold fields of Gate River and Marceba, and the Coal Measures of Rockhampton. Mr Jack in his report mentions the occurrence of coal-seams at various localities near Brisbane, and regards the district as likely to prove payable. We have also received two reports by W. H. Rands on the Eidsvold Gold Field, with one on the Croydon Gold Field and one on the Horn Island (Torres Straits) Gold Field. Mr Jack reports on the Hodgkinson Gold Field and the Brovinia (or Brorinia) Gold Field. From a note by Mr Jack on traverses of the Bunya Bunya Range we extract the following sentences of more purely geological interest:—

"The road from Macalister to Jimbour lies over black-soil plains which the previous day's rain had made very heavy. The black soil is evidently derived from the detritus of the basaltic high

grounds, which are first met with at Jimbour. It is probable, however, that the sandstone and shales of the Mesozoic Coal Measures immediately underlie the soil beneath the road. At Jimbour, near the edge of the basalt, a coal-seam is met with at the depth of 60 feet. . . . The basalt is disposed in several thick beds, which once formed continuous lavaform sheets. They dip to the south-west, or perhaps, more correctly speaking, have been spread out over a surface gently sloping in that direction. They might at first sight be supposed simply to overlie the Triasso-Jurassic Coal Measures, and therefore to be of Tertiary age; but as we occasionally find sedimentary strata of similar character to those of the Coal Measures interposed between two beds of the basalt, there can be little doubt that the latter are older than the outbursts of Tertiary lavas, and in fact contemporaneous with the Coal Measures. There are, however, strong grounds for suspicion that the basalts had suffered considerable denudation before the deposition of the sedimentary rocks west of Jimbour; that, in fact, there is an unconformity within the Trias-Jura area. A great thickness of the basalt beds is met with near the centre of the 'range,' whereas at Jimbour and the other western outposts of the denuded basalt only the lowest bed is seen. . . . It is evident from the sections of Ironpot, Boughyard, and Jumma Creeks that, as the base of the volcanic series (basalts) rests directly on the granite, the whole of the immense thickness of sedimentary beds lying between the Toowoomba basalts and the Brisbane tuffs is locally absent. In other words, there is what is called in geology an 'overlap.' The base of the Triasso-Jurassic series as seen at Brisbane is on a much lower horizon than the base of the same series as seen at Ironpot Creek. The lower beds of the series had to be built up so as to fill up existing depression in the surface, before the upper beds were deposited at such an altitude as to cover the higher portions of the old surface."

Disease in Lilies

From the U.S. Department of Agriculture (Division of Vegetable Physiology and Pathology, Bulletin 14) we have received a preliminary account by Mr Albert F. Woods of investigations into the cause of the spotting and distortion of the leaves and flowers of lilies, which have given much anxiety to American horticulturists. He believes the disease to be due to a combination of causes—a weak stock of bulbs, careless cultivation, and the attacks of mites, aphids and fungi. It is hoped that a rigorous selection of bulbs for propagation and a more scientific system of culture will check the progress of the disease. It is startling to read in a publication of this department that "the bulb mite (*Rhizoglyphus echinops*) is a white, sluggish insect with brown legs."

595.78

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The Classification of the Day Butterflies

IN classifying the day butterflies systematists have availed themselves of external characters, as a rule without subjecting these to a test, by way of ascertaining their comparative value. Now, each organ, or part of an insect, presents taxonomic character relatively stable and primary, and again relatively unstable, recurrent, or adaptive and secondary. It is a peculiarity of the secondary characters that they engage the attention, only in the end to disappoint our efforts to use them in establishing a system,—one which shall not constantly leave exceptions upon our hands to be accounted for otherwise than by the system itself.

There exists, also, the prevalent belief, that there is really a structurally more advanced leader to that host of butterflies which is always pressing forward out of the future to appear in the present and vanish into the past, and that the rank and file of the butterfly phalanx emulate this favoured one, or its type, in structural perfection. A direction in the specialisation of this or that organ—yes; an ultimate goal which all are alike striving to attain and in which some are merely outstripping the others—no. Rank must always remain a relative conception, and, to avoid a confusion of ideas, it is better to substitute for it, in natural history, the terms *specialisation*, as denoting advanced, and *generalisation*, as signifying retarded development of organic features.

We can illustrate what we mean by taking up one or another of the systems which have been put forward as an arrangement of the day butterflies, and no better one can present itself than that advocated by Mr Scudder in his book on the 'Butterflies of New England' and elsewhere. Here the Satyrids, or 'Meadow Butterflies,' are placed 'at the head' (to use the characteristic phrase of the Philadelphia Check List), while the 'Swallowtails' are inserted between the 'Blues' and the 'Skippers.' That, through this proceeding, Mr Scudder has been led into the mistake of separating two groups, which belong phylogenetically together, by the interpolation of a third, not at all nearly related to either, we expect to prove. The mistake would seem to have been committed by Mr Scudder through a want of discrimination between adaptive or

secondary and primary characters, as well as by neglecting the lessons taught by a study of the neuration or veining of the wings. But first we will hear Mr Scudder:

"It is well known that as a general rule moths undergo their transformations to chrysalis within a cocoon, spun by the caterpillar, or in a cell moulded beneath the surface of the ground. The same is true of the lowest family (*i.e.* of Day Butterflies) Hesperidae, which usually make such a cocoon within a rolled-up leaf or cluster of leaves, and hence had given them by Boisduval the term Involuti. It was not noted by him, nor has it been, as far as we are aware, by any author, though figured by many, that within this cocoon they generally, perhaps always, spin a pair of shrouds, into the middle of one of which they plunge their cremaster, while by the other they support the middle of the body.

"Now, remove this outer cocoon and leave the shrouds, and one has, with only such changes as are absolutely required by the lack of the encircling cocoon, the character of the support of the chrysalis of the Papilionidae, *viz.*, a button of silk attached to the object from which the chrysalis hangs, and a loose girt around the middle of the body. In the Lycaenidae, we pass simply to a still closer attachment of these fastenings, so that the rounded chrysalis appears almost glued to the surface to which it is attached; and these two families, the Lycaenidae and the Papilionidae were classed by Boisduval under his Succincti. In the Nymphalidae, by the loss of the median girt the chrysalis hangs suspended by its hinder end, and forms the group termed by Boisduval Suspensi or Penduli, which he and his followers interpose between the Involuti and the Succincti. Yet we have here a regular progression from the cocoon of the moths to the almost total absence of the use of any silk of the quiescent period of life. Even the few exceptions to this rule seem to be entirely explainable as instances of reversion. Thus the only case among the higher butterflies where a cocoon properly speaking is made, is in the subfamilies most closely allied to the Hesperidae, among the group of Parnassidi and Anthocharidi; and again in exceedingly feeble instances where the necessities appear to be overwhelmingly great, among some of the higher Nymphalidae, which have lost even the last remnant of the cocoon of moths, *viz.*, in some of the Satyridae, which lack cremastral hooks and undergo their transformations ordinarily in the rudest form of a cell which they can construct above or at the surface of the ground, by the mere movements of the body and the spinning of one or two threads of silk."

I think, from the above extract, it is clear that Mr Scudder rests his classification upon considerations drawn from the peculiarities of the mode of spinning, *i.e.*, the preparations made by the

active larva for the protection of the quiescent chrysalis. When we admit that the habit of spinning a 'shroud' within the cocoon by *Hesperia* may represent a stage preparatory to an abandonment of the making of a cocoon altogether, we make Mr Scudder a very ample admission. Yet upon this basis Mr Scudder proceeds, ignoring striking and important differences in structure, to place the Hesperiadae next to the Papilionidae, implying that the papilionid chrysalis is the immediate phylogenetic successor of the hesperid, and, consequently, that *Hesperia* represents the next ancestral form of *Papilio*. Granting that the hesperid cocoon represents an ancestral, generalised stage, one which may have led to the waist-tied chrysalis, there is no reason why the papilionid waist-tied chrysalis should be the successor, seeing that there are other Succincti to be considered. But, for Mr Scudder and his system, this is not an objection, because both demand that all waist-tied chrysalids should be thrown together in one category. Now, the hesperid cocoon may as well have preceded the lycaenid waist-tied chrysalis, and here there are other and good reasons for believing that this may have been the actual sequence. For the character which induced Boisduval to class the Papilionidae and Lycaenidae together as Succincti, is, when we compare it with other characters, seen to be secondary only, one of convergence in habit, one not in itself by any means synchronous, one, finally, evidently reached upon entirely differing phylogenetic lines. The structural gap between *Papilio* and *Lycaena*, is one of the widest among the day butterflies. The utmost we can grant to Mr Scudder is, then, this: that the mode of attachment in *Hesperia* may represent a stage by which the cocoon-making larva prepared itself to abandon the habit. To make more of the observation than this, is to trifle, to exaggerate. And, in itself, the view that the waist-tied chrysalis is a further and necessary stage between the cocoon and the suspended chrysalis seems erroneous, inasmuch as the former relapses sometimes into the cocoon, as in *Parnassius*. Elsewhere, in the Lepidoptera, the cocoon is apparently abandoned without giving birth either to the waist-tied or the suspended form. There exists, then, no such necessary series of changes as Mr Scudder's system should be able to fall back upon to give it stability.

Now, in *Zerynthia* or *Thais*, the girdle of the waist-tied chrysalis has slipped up to the head of the chrysalis, to the 'nosehorn,' as stated by Dr Chapman; so that if it went up any further, the pupa must fall over and, being then only fastened by the tail, would hang down and turn, so to speak, from a Succinctid into a Suspensid. But the girdle, in this case, although slipped over the head, would still be there, while useless as a support, and the general question would still remain of how the girdle had been gotten rid of to enable the

chrysalis to pass from the type of the Succincti to that of the Suspensi. Just as Mr Scudder finds in *Hesperia*, making a 'shroud' within its cocoon, the beginning of the Succincti, so in *Thais*, with its girdle slipped up to the 'nosehorn,' we may find prefigured the beginning of the Suspensi. In each case we are furnished with a hint and no more of the possible *modus operandi* by which the cocoon and the girdle have become discarded. And if in the next more specialised butterfly than *Thais* we found a Suspensid, we might have Mr Scudder's ideal sequence actually realised. As a matter of fact *Parnassius*, which *is* this butterfly, walks back into the cocoon, and what becomes of the slipping girdle of *Thais* we, that is, I, cannot say. Perhaps these or similar considerations might have occurred to Mr Scudder; but, when one has so beautiful a theory in hand as is supplied by the 'shrouds' of the Involuti, one may be forgiven for not looking deeper into the matter. Yet it is one deserving of a little thought.

Long ago I called attention to the separate nature of the conditions of life attending the larva, pupa and imago, and drew the corollary that these different stages must be studied by themselves, and judged by their own standards. The caterpillar and moth of *Apatela* go their separate ways, having different cares to meet, and what in each is a relatively primary character must be distinguished from what is a relatively secondary. In one sense the larva dies when it changes to a chrysalis, and the chrysalis perishes when it discloses the butterfly. Both throw off their former mode of life and its dangers with the cast skin.[1]

It is probably, then, a fallacy to believe, with Mr Scudder, that there is 'a regular progression from the cocoon of the moths to the almost total absence of the use of silk,' and that the facts imply such a progression. Instances are not rare where the generalised forms spin little or no silk, and the specialised forms, on the same line, make large and complex cocoons. This envelope to the pupa is so clearly an adaptive character that in one, single, upon other characters quite homogeneous group, like the 'Emperor Moths,' it runs through the entire scale, from utter absence to a specialisation seldom attained throughout the Lepidoptera, the hanging cocoons of *Callosamia* and *Attacus*. Mr Scudder himself speaks of cases of exception to his theory, 'where the necessities appear overwhelmingly great.' So that a 'biological necessity' exists, at least sometimes. But the necessities are always great, being ultimately matters of life or death. In the 'Emperor Moths' it is the more generalised forms that spin no or little silk, and the more specialised that secrete

[1] In my original communication, *Mittheilungen aus d. Roemer Museum*, No. 8, p. 15, I have shown that the pupal specialisations given by Dr Chapman for the Pierinae do not run parallel with those offered by the neuration of the imago.

much silk and make complicated cocoons. Mr Scudder regards as a logical sequence the class of facts offered by *Hesperia*, making a 'shroud' within the cocoon, by *Pieris*, dispensing with the cocoon, by *Nymphalis*, hanging only by the tail and losing the 'shroud,' and finally by *Oeneis*, losing all attachment and transforming almost nakedly among the surroundings of its bleak home. But only on paper does this series figure as a scheme of progression. The isolated occurrences are brought artificially into connection by Mr Scudder as a basis for his system. The separate and recurring features of adaptation are not phylogenetically related. Nor do the specialisations of the butterfly keep pace with this imaginary series. *Pieris* has a more specialised neurational pattern than *Nymphalis*, and *Nymphalis* than *Oeneis*. As to the spinning, what evidence we have rather points to a reversing of Mr Scudder's sequence, since the more complicated the pupal envelope, the more specialised the butterfly appears often to be. The Succincti, as a phylogenetic group, has no reality behind it, because it is composed of butterflies specialised upon different primary patterns of wing structure. So that the elements out of which it is made up are in reality foreign to each other, and are only brought together upon a character which has been evidently separately evolved, the common origin of which is contradicted by the disparate structure of the 'Blues,' 'Whites,' and 'Swallowtails.'

Let us observe the way these schemes arise and are perpetuated. No sooner had Boisduval noted the differences in the manner of attaching the chrysalis, than he made a system out of the several modes, and invented Latin names for the resulting categories. M. le Dr Boisduval (alas! that I write in the past tense) was *bonhomme*, well satisfied with himself and the world, and, after launching his idea in its scientific dress, troubled himself not at all with its correspondence, or want of correspondence, with the entire facts it was designed to cover. It was as if the attractive and careless gaiety of French genius had presided over the fabrication of a system so manifestly *décolleté*. The net of Boisduval's classification is, however, sufficient to hold Mr Scudder, although the butterflies themselves may escape through its meshes. And Mr Scudder, surveying the Lepidoptera through its medium, supplies us plentifully with reasons why such a net should be the one thing needful to interpose between the butterflies and our understanding of them. It is true that Boisduval's system possesses both effectiveness and a certain ease which charms the mind, but when it is pieced out by argumentation, when it is used as a basis for further scheming, we come to see how insufficient it really is. Boisduval himself belonged to the old school and preferred *Papilio*. It was therefore rather an unkind use of this author's pupal system to forge thereout weapons against the ancient order. But I believe they will turn in Mr Scudder's hands.

To the French school, to which we owe so much which has made Entomology attractive, we are moreover indebted for that divorcement of the butterflies and moths which is pronounced by the terms Rhopalocera and Heterocera. Convenient as these terms may be to designate what is a rough and surface grouping, we must always be led to reject them as indicating, by means of categorical names, a structural difference greater than really exists. In coining these titles M. Boisduval stepped into the slippers of M. Dúmeril, and has since found imitators who have invented for us the additional names of Nitrocera or Grypocera. All this brilliant play at making systems, without weighing all the results, fascinates many, and among these also Mr Scudder, who remains among the Suspensi and Succincti, weaving in an additional original observation on the 'shrouds' of the Involuti, and believing for ever that upon these threads hangs the history of the butterflies. It is almost a pity that we are compelled to remember that systems and categories are but part of the machinery of science, and that when they over- or under-do their appointed work (which is here to designate the affinities of organisms) they should in reason be changed or even swept away.

At what time we may imagine the primeval and colourless lepidopterous caterpillar to have left its hidden home in the stems and leaves of plants and emerge into the open, here, under fresh conditions, taking up the struggle for existence, and dodging the effects of its difficulties by adapting itself to them, thus finding in the conflict the way for the endless variation we find in the coloured larvae of to-day —at that time, also, did the primeval caterpillar bring with it, of necessity, the habit of spinning silk to provide for the safe repose of the chrysalis. This protection would naturally vary with the surroundings. Impeded by circumstance, subject to be thwarted and turned aside by different objects and new conditions blocking the fresh roads of travel, the habit of spinning would express itself unequally, the old fashion displayed would become modified, at times the original custom might even altogether fall away. And this is what we see—cocoons of all sizes and consistencies, to the Noctuid fastening together grains of sand with silky saliva. Even now the individual spinning larva, forcibly prevented from completing its web, or from attaching itself, will transform without protection, and trust to fate to live and become a butterfly. So that the mode of spinning is seen to be in itself secondary and auxiliary merely, and we can understand how identical or similar fashions might appear upon independent phylogenetic lines.[1] Now, however the fashion of

[1] Among the Bombycides, the Lachneidae spin a cocoon, the nearly related Craterony-gidae pupate nakedly in the ground. My friend, Prof. Dr Pabst, writes from Chemnitz of a certain larva of the cocoon-making *Euthrix potatoria*, that it transformed nakedly upon the surface, without spinning a thread, yielding in due time a normal imago.

spinning might tend to crystallise and be sedulously followed, it would no less be liable to give way on occasion to change, to adapt itself to new circumstance, even to break more or less suddenly with custom, and this quite independent of the modifications of the butterfly. Undoubtedly, the mode of spinning has its story to tell, and we should be at the pains of reading it rightly, and use the information as a help to a phylogenetic classification; but, from its very nature, the character cannot be drawn out to the extravagant extent attempted by Mr Scudder. What, for example, are we to think of the Geometrid or the Plume which suspends its chrysalis after the fashion of a day butterfly? Mr Scudder can only answer: 'It is not a butterfly,' and dismiss the case. But we think that it is rather time to discard the use of Boisduval's terms of Suspensi and Succincti and Involuti as designating natural groups, and cease to rely upon the characters drawn from these assemblages as affording us a key to the phylogenetic classification of the butterflies.

Beyond this, we can show that Mr Scudder's estimate of *Oeneis* takes no account of the structure of this butterfly's wings. From these organs it appears clear that *Oeneis* is not a specialised form, one in any way remarkable above its fellows. The most specialised Satyrids I have yet examined in the European fauna appear to be *Pararge aegeria* and *Lasiommata megaera*. In these vein IV_3 of the hind wing has effected its complete junction with the cubitus, reproducing a character normal with the Nymphalidae proper. Now *Oeneis* has not this character at all; here, as in all Agapetinae, to which sub-family group it belongs, vein IV_3 springs still from the cross-vein. *Oeneis* is plainly allied to *Erebia*, even a little more generalised, as shown by its completely confined median cells, the space between the veins more equidistant, vein IV_3 a little further from V_1, the fusion of II and III hardly so extended. Among the members of its family we have in *Oeneis* by no means a 'high,' rather a comparatively 'low' butterfly; so that it seems absurd to place such a butterfly 'at the head' of the whole system, because it proceeds from a nearly silkless chrysalis. And we may conclude, with Hume, that when any opinion leads to absurdity it is certainly false.

In Mr Scudder's paper on the "Classification of Butterflies, with special reference to the position of the Equites or Swallowtails," Philadelphia, June 1877, we find a variety of reasons assigned to justify the deposition of *Papilio*. Those upon which Mr Scudder seems to lay most stress, we notice here: "But perhaps the most striking point of affinity between these two groups (*i.e.*, 'Swallowtails' and 'Skippers') lies in the possession, on the front tibiae, of the characteristic foliate epiphysis which is wanting in all other butterflies" (*l.c.* 77). Now, did this character occur only in these

two groups, of all Lepidoptera, it might be conceded to be a mark of affinity, but since this is not the case it may either be continuous in the two groups from a remote ancestry, before either *Papilio* or *Hesperia* were thrown off, or it has been independently acquired. Since it is not a relatively primary character it affords no conclusive argument for the sequence adopted by Mr Scudder. Again: "The inner border of the hind wing is folded longitudinally just as it always is in the 'Skippers' and rarely in other butterflies." This is a secondary character and the same sort of argument would bring *Parnassius* and *Rothschildia* together, for both have the inner margin of the hind wings hollowed out, and here vein VIII has disappeared in both instances. In the Lepidoptera, quite generally, the inner border of the hind wing becomes variously specialised. "But again," says Mr Scudder, "the Swallowtails are universally conceded to be so closely allied to the Pierids, that they are invariably placed next them; consequently, if the Swallowtails are placed highest in the scale, the Pierids must go with them; nobody questions this; yet the Pierids possess not a single one of the characteristics by which a high rank is claimed for the Swallowtails. Commentary upon this is needless." Now, I believe myself that commentary upon this criticism of Mr Scudder's is 'needless.' The whole remark is based upon the existence of affinities between the 'Swallowtails' and the Pierids, which I decidedly question, and loses its point. The resemblances with the Pierids lie in the parallel secondary movements of the movable veins. The phylogeny of the two groups is most clearly distinct. *Papilio* is not so much a 'low' or 'high,' as a peculiar butterfly. The conclusion Mr Scudder comes to with regard to the *osmateria* of *Papilio* is, that because a similar character is developed in *Cerura*, which is a moth, it must indicate low rank in the butterfly. But this is a character of secondary larval adaptation, in which *Cerura* far outstrips *Papilio*, and by the same token shows the inequality of specialisation, and that this character in the larva, being secondary and adaptive, cannot directly affect the imago. It may seem extraordinary to Mr Scudder that a moth should be in any point 'ahead' of a butterfly, but such nevertheless is the fact. Mr Scudder's view of the case arises from the fundamental error that there is a linear succession in 'rank,' which, of course, precludes the idea that in any character a moth could outstrip a butterfly. Again, when we admit, with Mr Scudder, that the straight ventral surface of the abdomen in the pupa of the Suspensi is retained over from the Succincti, it does not necessarily indicate that the Suspensi outrank the Succincti, but it makes it probable that the brush-footed butterflies had their origin in the main stem of the six-footed butterflies after the waist-tied habit had been acquired. The Pierid Succincti have gone their own way after

thus probably, at some point in their history, having given off the Nymphalid Suspensi, and to-day their wings show specialisation in both of two directions, while the brush-footed butterflies only show it in one. The plain palpable fact, that the wings of *Papilio* are laden with characters contradicting those of *Hesperia*, is ignored by Mr Scudder, who pursues after characters, either fancied or of less or misunderstood value, to assist his theory, which seems to him to demand that the two groups be brought together. Alone the position of the radial branches, notably that of III_4 and III_5, make it extremely improbable that *Hesperia* and *Papilio* should be at all nearly related. Reasons may be far-fetched and resemblances deceptive, but, when we come upon characters which forbid, we may be sure our schemes ought to be dropped. We cannot, with Mr Reuter, bridge a difficulty with a Latin name.

There exist likewise a number of published arrangements, beside Mr Scudder's, in which the 'Blues' are brought in next to the 'Whites,' and, as is oftenest the case, without any assigned reason. Not unlikely is this course, in some instances at least, due to a recognition of the fact that in both groups a diminution in the number of radial branches occurs. But this reduction takes place also in the Parnassiinae and in the Saturniades as well, and is clearly a recurrent, secondary movement, following a general direction in evolution. As such, in the absence of other grounds, it constitutes no sufficient basis for bringing the 'Whites' and 'Blues' together, and cannot be properly used for such a purpose in classification. But to review all the proposed arrangements of the day butterflies would give us a volume as bulky and difficult to manage as that recently issued by Mr Reuter. It will be sufficient if we have made it clear that the nature of the class of characters, used by recent systematists to establish their sequences, has frequently not been well understood.

Before giving a summary of the results at which I have arrived from the study of the neuration, it may be well to give the method employed. This consisted in preparing enlarged photographs of all the wing patterns to be studied. By placing these impressions side by side upon shelves where they could be shifted, comparison could be readily effected, and, in this way, the difficulty of examining each specimen by itself under a microscope and carrying the picture of what was to be noted in the mind, was obviated. Sketches and drawings are here of little use, while almost all the published drawings I have been able to compare are not sufficiently accurate to be relied upon for study.

And before enumerating the detailed observations upon which my conclusions are based, I may epitomise these latter as follows: The system of Bates, which places the brush-footed butterflies 'at

the head,' in a linear arrangement, is not especially warranted by the neuration, since here the radius remains in a five-branched generalised state, none of the branches having submitted to extinction by absorption. The Parnassians and 'Swallowtails' possess exclusive neurational features which tie them together, and authorise their being regarded as a morphological group, equal in value to all the other butterflies. Hence they cannot be properly interpolated at any point among the latter. Nor can they be placed after the Hesperians, because the looped vein VIII on the primaries of the 'Skippers' is repeated in the Sphingides and other assemblages of the moths. Furthermore, the Parnassians constitute in the clearest way a specialisation of the Papilionidae, of which the steps can be followed, the Zerynthianae representing an intermediate stage between the Papilionidae and the Parnassiinae. Any sequence, either in letters or of an objective series of specimens, should commence with the Parnassi-Papilionidae. The 'Blues' are nearest related to the 'Skippers,' of which they form, in an analogous manner, a specialisation, although here the gap in other features outside of the wings is wider. The 'Whites' and the brush-footed butterflies are equally connected by a common pattern of neuration, however they may separate upon the structure of the feet.[1] I thus return, in the main, to the sequences of Linné in 1758 and of Fabricius in 1787.[2]

The drift of my discoveries and conclusions lies in the direction of the rehabilitation of *Papilio* and a correction of the estimate placed by Bates and his followers of the value of structural features in the diurnals. Mr Wallace and Mr W. H. Edwards, though their special arguments may be inconclusive, will have been justified after a long dispute. It might, indeed, have been thought sufficient to present *Papilio*, resplendent in colour, imposing in size, perfect in development, labelled in addition with a knightly name, to secure recognition. Anthropomorphic as are the actual terms of the presentation,[3] this is in so far valid, as the Parnassi-Papilionidae are structurally separable from all the other butterflies. But in this papilionid group it is not *Papilio* itself which is the most specialised, but its scion, *Parnassius apollo—cedant arma togatae !*

If we follow Mr Scudder, we must commence our sequence of

[1] Mr Bates' plea for *Heliconius*, in itself an otherwise generalised butterfly, but one in which the forelegs are specialised to an extreme, is a logical one under Mr Scudder's statement that "we must accept atrophy of these legs (!) as a conclusive mark of high organisation" (*l.c.* 79). Some moths (perhaps a fact not known to Mr Scudder, *e.g.*, *Pallachira bivittata*, Grote) have, however, also the front feet aborted. But since Mr Scudder passes *Heliconius* by, and prefers *Oeneis*, we are spared here the discussion.

[2] "Mantissa insectorum," Hafniae, Tom. ii. pp. 1-92.

[3] "The Malayan Papilionidae," etc., pp. 133-140, Am. Ed., 1871: "Owing to the complete and even development of every part of their organisation, these insects best represent the highest perfection to which the butterfly type has attained," etc.

the day butterflies with *Oeneis* and the 'Meadow Browns'; if Mr Bates, with *Heliconius*, classified incorrectly by Mr Reuter with the 'Wood Nymphs'; if Dr Skinner and the Philadelphia Check List, with *Danaus* and the Limnads. But, if anyone will follow me, then with *Parnassius* and the 'Mountain Butterflies,' bringing in their train *Papilio* and the 'Swallowtails,' and restoring these to their own again.

Leaving all subjective ideas severely alone, we may have succeeded in showing that the reasons brought forward by Mr Scudder to sustain the pre-eminence of the brush-footed butterflies are quite hollow. It appears to me that the day butterflies are, considered as a whole, relatively specialised Lepidoptera, and that any sequence of the order should commence with or culminate in them. Whether we are now able to settle the difficult question of development in this order of insects, or whether we are not, it seems to be equally proper to commence our arrangements with the diurnals, and this from practical considerations, having in view all the exigencies of the case. This being so, and appearing to be the common-sense view of the matter, I need not enter here into the difficult question of the comparative specialisations and generalisations of the moths. I wish merely to record my opinion that the book, catalogue or collection will best subserve the interests of science, and will have the most success, which, reckoning with practical and not theoretical and disputable considerations, makes its beginning with day butterflies, whatever special sequence of their groups be chosen. But no shadow of a doubt remains upon my mind—we should commence the series of the day butterflies with the Parnassi-Papilionidae.

A. Radcliffe Grote.

Roemer Museum, Hildesheim.

(*To be continued next month.*)

EXPLANATION OF PLATE I.

The figures are obtained by photographic process.

III = Radial veins; IV = Median veins; V = Cubital veins.

Fig. 1. *Parnassius apollo*. Type of genus and family. Size 1/1. The specialisation, as compared with *Papilio*, is to be traced in that IV_1 of the median series has left cross-vein, and that the position of IV_2 is cubital on both wings. In the second direction that the radial branches have become reduced in number.

Fig. 2. *Papilio machaon*. Type of genus and family. Size 1/1. The character binding the group Parnassi-Papilionidae together is seen in the retained vein IX on internal margin of forewings, the hollowed out internal margin with vein VIII lost, and vein VII shortened, especially in *Parnassius*. The generalisation of *Papilio* is shown in the retained intercalary vein marked VI, the central position of IV_2 on primaries, the five-branched radius. On hindwings the separation of vein I must be noted, showing that this so-called 'praecostal spur' is really a longitudinal vein, which becomes basally absorbed by II in *Parnassius* and most other butterflies. It is partially distinct in *Limnas* and *Zerynthia*.

FIG. 3. *Hesperia malvae.* Type of genus and family. Size 2/1. All veins separate, relatively equi-distant. No vein IX; instead, a loop at base of VII, as in other Hesperiades (when not worn out). Introduced here to enable the beholder to see at a glance the impossibility demanded by Mr Scudder's classification.

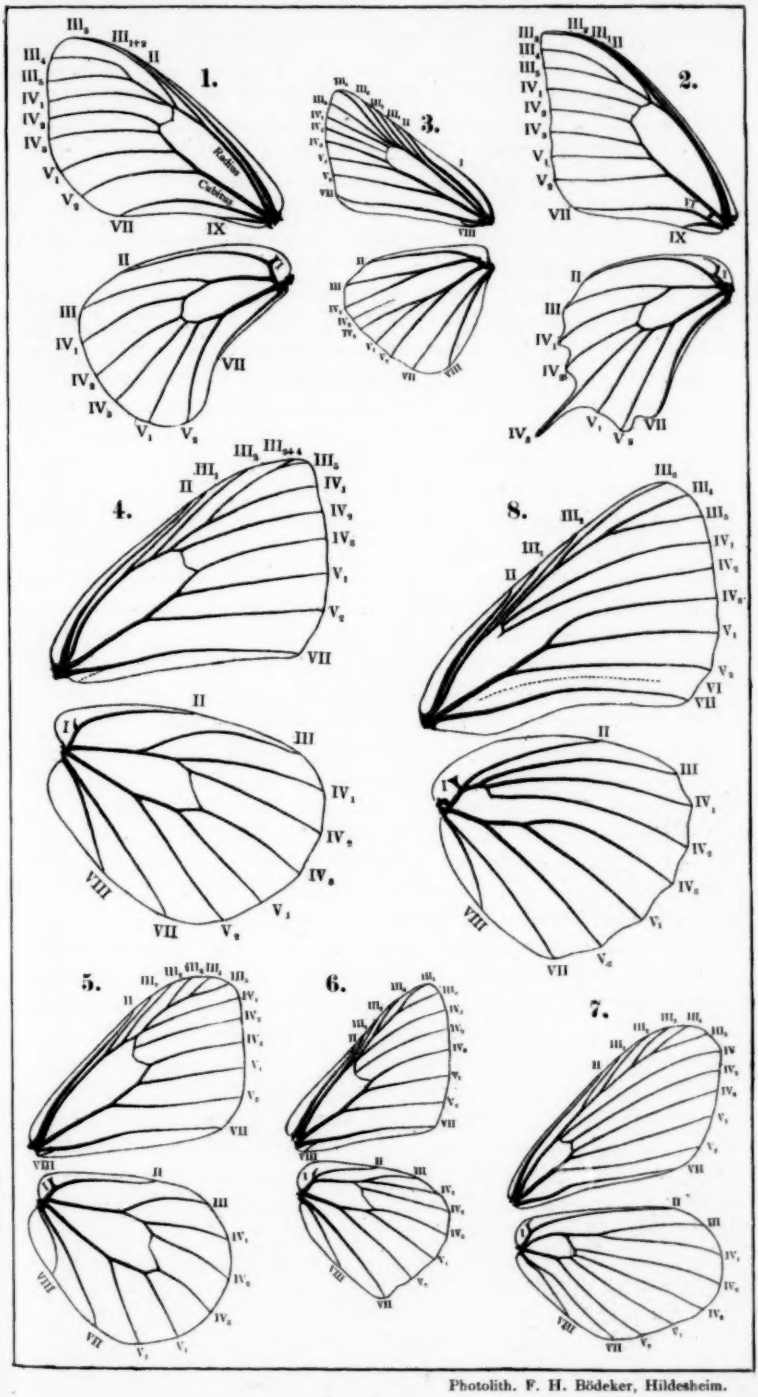
FIG. 4. *Pieris rapae.* Type of genus and family. Size 2/1. The small partially absorbed vein III_{3+4}, vanishes in *M. brassicae.* On hindwings note want of absorption of III by II and position of cross-vein.

FIG. 5. *Euchloe cardamines.* Type of genus of Anthocharini. Size 2/1. Note the five-branched radius, generalised as compared with *Pieris.*

FIG. 6. *Nemeobius lucina.* Type of genus and family. Size 2/1. Introduced here to show correspondence with Pieridae. On secondaries vein IV_1 leaves radius (vein III) beyond the cell. Compare with figures given here of Lycaeni-Hesperiadae.

FIG. 7. *Leptidia sinapis.* Type of genus and sub-family. The disparateness between this and the other Pieridae here figured is to be noted and text compared.

FIG. 8. *Nymphalis lucilla.* Type of genus and family. Size 2/1. The cell is open, the system of the media has disappeared from the wing as a system, IV_1, an IV_2 have joined the radial, IV_3, the cubital system. II and III fused on hindwings to point of issue of I.



Photolith. F. H. Bödeker, Hildesheim.

A. RADCLIFFE GROTE. WINGS OF BUTTERFLIES.

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II

The Authenticity of Plateau Man: A Reply.

IT is not to be expected, when a discovery of new facts is announced revolutionising any branch of science, that it will be welcomed and accepted by all. There is no pain so great as the pain of a new idea, and the view that man was entirely a Post-glacial mammal had become such an article of faith on this side of the English Channel that the evidence must of necessity be very strong to account for the recent change of front. For this change we are entirely indebted to Mr Benjamin Harrison and the late Sir Joseph Prestwich, whose recent removal from our midst we all so greatly deplore. The marvellous industry and enthusiasm of the former supplied the facts from which the latter, with his wide geological experience and his unequalled knowledge of Palaeolithic flint implements, was enabled to prepare his papers on Plateau man. Since the publication of these, additional evidence has been adduced by Mr W. J. L. Abbott and Mr R. Bell, and the existence of man in England in Pre-glacial times has now received the assent of nearly all competent geologists.

It is noteworthy that the most enthusiastic supporters of this view are to be found in the ranks of the "flint hunters" as opposed to the mere collectors. The reason for this is not far to seek. The former's knowledge is acquired in the field; the latter derive theirs from museum collections. In the matter of flint implements nearly all our museums are terribly misleading. After a careful examination of, say, the collection at Bloomsbury, the uninitiated depart with the idea that Neolithic man only used tanged flint arrowheads and ground celts, varied with an occasional flint knife or scraper, but all of them beautifully worked; whilst his predecessor, the Palaeolithic savage, made nothing except ovoid and pointed implements, yet all of such a character that no one could possibly doubt their human origin. The collector, therefore, only knows the best implements, those which are commonly spoken of as "museum" specimens, and he only accepts from the dealer or the navvy the implements of this class (and when the supply is short it is easily augmented from certain notorious sources). The "flint-hunter," on the other hand, knows only too well that for one

good example he will find hundreds of indifferent ones. The rough, badly-struck flakes used as scrapers and knives, as their edges testify, the rude axes and other tools, both Neolithic and Palaeolithic, are familiar objects to him; for the ancient savage, except on rare occasions, was in the habit of making his tools and weapons with the least possible labour. Hence these trimmed flints from the Plateau naturally appeal to him; he has seen similar work in more modern specimens, and of its human origin he has no doubt.

It is remarkable that, until the appearance of Mr Cunnington's paper in the November number of *Natural Science*, no serious attack had been made on Plateau man. Adverse criticism, it is true, had been evoked on the flints at the Geological Society and at the Anthropological Institute. They had also been dismissed with "Of no interest whatever." Consequently it is a matter of congratulation that we are at last in a position to examine carefully the various objections and criticisms which, though often urged in private, have never appeared in print till now. Since, however, the Pre-glacial age as well as the human origin of the flakings has been attacked, it is necessary to restate the case for Plateau Man.

Scattered over the North Downs, but always at a great elevation, comparatively speaking, are patches of flint gravel, in which one also occasionally finds Pliocene ironstone and the well-known Oldbury stone. The flints are stained a deep ochreous brown colour, and some of them are chipped on the edges, whilst very rarely a fairly well-shaped implement is found. That these flints were at one time in an iron pan there can be little doubt, as portions of a ferruginous deposit are often found incrusting both the chipped and the unchipped portions.

Objection was first made that, as all the finds were on the surface, it was impossible to say anything about their age. By means of a grant from the British Association, pits were sunk at the Parsonage Farm, Ash, whilst last year further excavations were made by Mr Harrison. In all these pits, at a depth of 8 feet from the surface, a bed of gravel, varying from 6 to 12 inches in thickness, was found. The gravel was cemented by iron, and was so hard that a pick was needed to break it up. The section from the first pit, which is typical of them all, was as follows:—Humus, 1 foot; stony loam, 2½ feet; grey loam, with a few scattered pebbles and small worked stones, 3½ feet; ochreous loam, 1 foot; gravel layer, with many worked flints, 6 to 12 inches. The underlying sandy loam, which was pierced for a further depth of 19 feet, is probably of early Tertiary age.

Since the publication of Mr Harrison's finds on the North Downs the chipped flints have been found by other workers over a large extent of country. They have been discovered at Friston,

near Eastbourne, by Mr Hilton; at Stede Hill, near Lenham, by Mr Crawshay; at Blean Hill, Canterbury, Amersham, and Micheldever, Hants, by Rev. R. Ashington Bullen; near Salisbury, by Dr Blackmore; in South Essex, by Dr Frank Corner; and at West Wickham, Kent, by myself. With the exception of the Essex finds, the gravels in which they occur bear no relation to the existing river systems, whilst Dr Corner's specimens, though obtained from Thames gravels, are derived from older beds as their rolled and worn condition shows.

It cannot, however, be too strongly urged that photographs, however excellent as such, convey absolutely no idea of the appearance of these flints; and though the figures given in Sir Joseph Prestwich's papers, and in his last published work "On Some Controverted Questions of Geology," as also those given by Mr Abbott in *Natural Science* [4], are very good indeed, yet to obtain a true estimate of their character a careful examination of the actual specimens must be made.

In looking over a large series it will at once be noticed that the flakings differ very much in colour. On many of the flints from Branshatch the chipped edge is whitish or pale yellow; yet even on these specimens one may often find portions of the ferruginous deposit on the worked surface, so that probably the colour has been shown. Many of the examples, too, will be seen to be polished, and this Mr Cunnington attributes to the action of blown sand. But it is identical with that found on the flints from the Pliocene gravel of Dewlish, and considered by the Rev. O. Fisher to be probably "due to the long-continued percolation of water carrying fine silt with it"; and this view is, I think, the correct one. As to the scratches which occur but rarely, and which Mr Cunnington considers glacial, I think far too much stress has been laid on them. I cannot believe that they have been "caused by the movement of pointed flints across the flat surfaces of other flints under considerable pressure." The only effect of such conditions would be to smash all the points, the result which has occurred in all my attempts to scratch one flint with another. The scratches are perfectly clean and have certainly been caused by a mineral much harder than flint, and in all probability have been graved by the small quartz grains which occur in the iron pan. I should, however, like to point out that one implement, No. 534 of Mr Harrison's collection, is scratched on both sides, and this example has been accepted as of human manufacture by Sir John Evans. It is stained a deep ochreous colour, was found at West Yoke, and is now deposited in the Maidstone Museum, where I hope it will find a permanent home.

Mr Cunnington takes exception to the statement that, as a rule, the chips were struck off from one side of the flint only; but a

careful examination of my own collection, numbering nearly a thousand examples, fully bears out this view. This indeed is perfectly obvious to anyone who has tried to chip flints. A very large number of the Plateau flints are similar to No. 1, *Natural Science*, Vol. XI., Plate IX., namely, with one surface flat, and the other convex and rough. In order to chip an example like this the blow must be administered to the flat side, and we find that such is the case. At least 90 per cent. of the flakes on these flints have been struck off from the flat side, and in the remaining 10 per cent. it will nearly always be found that owing to the shape of the flint it is easier to chip from the convex side. In fact they are chipped in exactly the same manner as a reasoning being would chip them to-day, If they had been flaked by natural causes we ought to expect that the flakes would be forced off from either side in the same proportions, and this, as I have just stated, is not the case. As Sir Joseph Prestwich has remarked, "the trimming, slight though it may be, is to be recognised by its being at angles or in places incompatible with river drift agencies, and such as could not have been produced by natural causes which tend to remove all projecting points."

Mr Cunnington has entered very minutely into the history of four of the Plateau flints, and lays particular stress on the fact that No. 1, Plate IX., exhibits surfaces of four different ages. But as two of these would be admitted by everyone as non-human, there only remain two sets of flakings. He further states that, as "the chippings were not all formed at one period, this fact is quite inconsistent with the theory that they were artificially shaped by man." Perhaps Mr Cunnington only wishes this remark to apply to these old flints, because it notoriously does not apply to Neolithic and Palaeolithic forms. It is very common to find Neolithic flakes which have been twice used, the original surface being white, and the newer work black. I have seen many Neolithic scrapers, especially from the Wealden area where flint is scarce, which have been made from Palaeolithic implements. Work of two and even three distinct ages is by no means uncommon among the River-drift examples. It certainly seems to me more reasonable to suppose that a flint was twice used by man than to imagine that it has been subjected at two different periods to the action of 'natural' causes operating in a manner which would be absolutely unique, and which cannot be paralleled anywhere on the globe at the present time.

Another objection is to the supposed uselessness of the shapes. But because we cannot in all instances understand their uses, it by no means follows that they were without use. An extremely common form in the River-drift is the well-known tongue-shaped implement, but for what purpose it was employed is still a matter of

dispute, because at the present time no similar form is known to be in use by any savage people. With Neolithic specimens, what one authority calls a sacrificial knife (which by the way has generally a blunt edge), others tell us is a skin scraper. The fact is that little or no attention has been paid to savage wants and customs, and I am afraid that owing to the spread of civilisation it is now too late in most cases to obtain reliable information.

Mr Cunnington's statement that "the chipping is limited to the edges of the slabs: there are no known instances in which the flint has been artificially flaked into the form of the weapon: the asserted human workmanship is limited to chipping of the edges of naturally-shaped flints," is totally at variance with that of Sir Joseph Prestwich. "It is only in a very few instances that an implement has been entirely wrought out of a larger flint." As a matter of fact they do occur, but only rarely.

Another objection is the supposed abundance of these worked flints, and we are further told that "on the chalk plateau the stained flints are all more or less chipped." That there are large numbers of them in various collections is indeed true, but the vast majority have been obtained from the labourers, nearly all of whom are very keen hunters indeed. I have tramped many miles in search of them, and I should consider fifty examples found after six hours' search, as a very good day's work. There are patches of the gravel where one cannot find a single worked flint. If they occasionally occur in abundance at one spot, do not palaeoliths also occur in the same manner? I have found more Palaeolithic implements and flakes in one field as the result of four hours' search than I could carry away. I have never been so fortunate with Plateau specimens. Mr Cunnington states that "if the flints were worked, used, and then thrown down again, we should expect to find them widely scattered." Speaking for myself, I expect nothing of the kind, for it is well known to all geologists that the surface of the North Downs has suffered greatly by denudation in modern times, consequently the flints are not likely to be found on the wasted portions, but we do find them scattered over the surface of the unwasted portions. As to whether all the flints obtained from the pits show traces of human work, I am not in a position to state; but I must admit that I would reject a certain number, which, however, Mr Harrison accepts. The large bruised and battered specimens are to my mind the result of river action, but there are a large number which I unhesitatingly accept.

Then, Mr Cunnington urges that the hollows in the flints are chipped in the same manner as the edges, and points to this as a proof of the natural origin of the flakings. I have carefully gone through my own collection, and through a large part of Mr

Harrison's, and in no instance can I find a hollow the edges of which have been chipped. In some instances they were battered, but never chipped. We are further told that "the earliest chippings are the largest, as might be expected, since the conditions were the most rigorous. As the climate became milder, the forces that acted on the flints became feebler, and the chips removed were therefore smaller." This appears to me to be the opposite of what one ought to expect. The first chips would be small; they would increase in size as the temperature was lowered, until the maximum cold was reached, and they would decrease in size as the cold diminished.

Mr Cunnington, however, sees that it is of no use invoking the aid of river action to account for the flakings, so he postulates a frozen gravel, for which seemingly he invokes the remarkable property of softening flints. For we read that "pebbles in the gravel would be pressed against the upper edges of the flint slabs, and force off small flakes"; and then we are informed that "the chipping was due to pressure by some yielding material." The means, however, by which flint pebbles were converted into yielding material are not stated. Nor are we enlightened as to how a "yielding material" could exert sufficient pressure to chip so hard a substance as flint. Especially when it has to be borne in mind that the amount of force brought to bear in the form of simple pressure in order to effect such an object must be vastly greater than that which would be required in the form of a blow or tap. The action of this frozen gravel, moreover, we are asked to believe, was even yet more remarkable for having chipped the flints on one side; it turned them over and chipped them on the other, whilst it removed flakes from the thicker parts, and left the thin edges untouched. It made bulbs of percussion, and then by another pressure produced an *éraillure*. It chipped dozens of flints into exactly the same form; it carefully avoided breaking the pointed portions of these flints, however delicate they might be; and it even produced forms which experts have pronounced to be genuine implements. Lastly, this yielding material completely disappeared, leaving its work as the sole evidence of its existence, and no man has ever seen its like on this earth since!

There are thus two views between which to choose. The one held by the larger number of observers that these flints have been chipped by a reasoning being for a definite purpose, is proved by the fact that they admit of being grouped according to certain patterns. On the other hand there is the view advocated by Mr Cunnington, that they are the chance products of natural forces acting in a manner which cannot be matched at the present day. I for one certainly hold that the former is the more reasonable view. No one

would think of challenging the statement of Sir John Evans "that the rolling and wearing of the edges were probably caused by natural agencies." It is the occurrence and manner of the flaking that is relied on as proof of the human origin of these implements. The old and oft repeated challenge by Sir Joseph Prestwich to the doubters to produce natural flints having the form of the Plateau specimens has not yet been responded to, and I venture to believe it never will be.

As to the age of the gravels in which these implements occur the bulk of the evidence is of such a technical character that it would be quite out of place in this paper, but should anyone wish to examine it in detail I would refer him to the papers by Sir Joseph Prestwich. His view that these gravels are of Pre-glacial age has received the support of practically all geologists who are well acquainted with the area. Mr Cunnington holds them to be frozen gravels and therefore glacial, and this view has the support of Mr Clement Reid.[1] Mr Reid arrived at this conclusion after a brief visit to the district, and he would further correlate them with the implement-bearing gravels of Sussex and the Thames Valley. The only sections of the gravel that we know of are from the pits at Parsonage Farm, Ash; and here it was six to twelve inches in thickness cemented by a ferruginous deposit. Anything more unlike a frozen gravel than this I do not know. An undoubted frozen gravel deposit does occur in the area of the North Downs, as was pointed out years ago by Charles Darwin. It is to be found filling up the bottoms of the chalk valleys, and sometimes attains a thickness of thirty feet. It consists of unrolled and subangular flints, Tertiary pebbles, and other material, and is without doubt the sweepings of the adjoining higher land. Bones of the musk ox, mammoth, and horse have been found in it, as well as land shells. Derived Plateau flints also occur, thus clearly showing that they are anterior in age to this deposit. They also occur as derivatives in both the Thames Valley and the Limpsfield gravels. From its situation the latter has always been held to be of greater antiquity than the former and this view is supported by the character of the contained flint implements. In fact the whole of the available evidence strongly supports the Pre-glacial age of these beds, and none is forthcoming to uphold the opposite view. Mr Reid suggests that the rude character of the implements arises from the fact that owing to toughening by weathering these flints could only be battered and not flaked, but this ingenious suggestion has been easily disproved by Mr Harrison, who has made from the Plateau flints very good copies of the River-drift types, one of which is before me as I write; and he informs me that there is no difficulty in so doing.

[1] Annual Report of the Geological Survey of the United Kingdom, 1897, p. 78.

Without wishing to lay undue stress on the value of authorities, it may not be amiss to mention that amongst those who support the human origin of these chipped flints may be numbered Canon Greenwell (who says that he "knows of no natural agency which has, or indeed could produce, the signs of work so abundantly shown upon them"), Dr Blackmore, Mr E. T. Newton, Mr Allen Brown, Dr Frank Corner, Dr Sturge, Mr W. H. Holmes of the Smithsonian Institution, Professor T. R. Jones; many others too numerous to mention, have all acknowledged their human origin, and even Sir John Evans himself has accepted several. Mr Harrison could furnish a long list of names of those who have entered his house unbelievers but who have departed converts. Twenty years ago, Plateau man was unknown; to-day we are nearing the end of the battle; and twenty years hence he will be held to be an established fact and the world will wonder that his existence should ever have been doubted.

In concluding I must thank Mr Harrison for the great assistance he has rendered in the preparation of this paper.

A. SANTER KENNARD.

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NOTE.—The above list does not pretend to be a complete bibliography, but it contains all the papers necessary to the student.

III

A New Scheme of Geological Arrangement and Nomenclature

Part II

IN a previous paper (Vol. XI., p. 321) I laid down two propositions which I think will be more considered by the geologists of the future than they have been by the geologists of the past. One is that submarine deposits should be treated separately from subaerial ones, and arranged in sequence in a separate group; the second is that deposits should not be arranged or studied in the accidental sequence in which they occur in any particular locality, but rather in a series representing the life-pedigree of their fossil contents.

This does not exhaust the new departures which seem to be heralded by the gloaming light in the geological horizon. I have always felt that in those inquiries which are not strictly empirical, inquiries in which history in the wide acceptation of the term has a place, and which deal with the sequence of events as well as with experiment, that the true road begins with what we know most about, namely, to-day, and proceeds through gradually more encumbered and difficult ground into the mists and fogs that circle round the beginning of all things. It has always seemed to me that in studying archaeology, ethnography, comparative linguistics, &c., &c., our real starting-place ought to be to-day, and that our ultimate goal, the *terminus ad quod* to which we advance, is the primitive fact. This assuredly ought also to be the case with geology.

What is the use of framing hypotheses about times and conditions which are so foreign to our experience that every statement involves a dozen guesses and a tentative conclusion, until we have carefully mapped the intervening landscape? It will be said that this is mere pompous rhetoric, since Lyell preached the same sermon in better words. I don't think he did. Lyell, no doubt, applied the methods employed by Nature in her current operations to unravel the mysteries of Nature's acts in the past, and he converged upon such current methods a great deal of necessary and of useful attention. But he never seems to me to have looked upon the actually present condition of things, the actually living animals

and plants, and the landscape in the process of being actually moulded, as being in every respect a part of the geological record just as much as the Carboniferous beds that underlie the chair on which I am sitting at this moment. Things as they are at this moment represent the very last chapter in the history of the earth, and we can no more separate this chapter from the rest than we can tear a man from his shadow. It is with to-day that we must begin if we are to make any real progress in geological reasoning, and we must reverse the teaching of the orthodox museum-naturalist who raises a great fence between the dead past and the living present.

Again, another heresy, or rather a reversion from the orthodoxy of to-day to the orthodoxy of fifty or sixty years ago. Reasoning, we have been continually taught, is either deductive or inductive. To some of us deductive reasoning, which is the especial glory of the great German mind that fills our firmament just now with its appalling clouds, is a barren mother, or, if not barren, is a mother that produces monstrous offspring too often to give us any confidence in her results. To frame a splendid postulate, far-reaching, uncompromising, audacious, and then laboriously to make God's handiwork in Nature witness to the superior genius of Man, or, in other words, to bend and twist our facts in order to reconcile them with the *obiter dicta* of some Pope of Science, this is the epidemic from which we daily suffer more and more, and against which the natural instincts of a young and aggressive natural scientist as naturally revolt. We hate all kinds of *à priori* reasoning in a science like ours. It is not English, it is not rational; it is German, and it is metaphysical; and metaphysics, as some of us have found who have spent years in quest of its secrets, is like drinking Bass's beer out of an empty mug, or making love to the Venus of Milo. We abominate every kind of general postulate springing from the innate prejudices of superior men. We include in this the great fetish of the modern geologist, the doctrine of Uniformity.

The word 'uniformity' has done infinite harm in Geology from the confusion involved in its double meaning. In one sense every man of science is a 'uniformitarian.' Science is very largely the discovery or verification of law in the Universe, and the very first and most elementary of its axioms as confirmed by universal experience is the Uniformity of Nature's Laws. We none of us doubt that with the same causes the same effects will follow. We none of us doubt that the same quantity and quality of force acting upon the same materials will produce the same quantity and quality of result. In geology, however, the term 'uniformity' has acquired an entirely different connotation, which is as illogical and mischievous as it is scouted by the students of other sciences. Lyell, following the guidance of Hutton and of Playfair, converged a great deal of

thought upon the various current methods in which the earth's surface is being disintegrated and renewed, and having calculated the cumulative effects of very small causes extending over very long periods of time, he was more or less fascinated by the result, and argued sometimes, as if he thought that the whole architecture of the universe had been fashioned in this gentle way. The diurnal effect of raindrops, of the grinding of the tide, of the results of frost and heat, of wind and water, when supplemented by unlimited drafts upon time, were deemed competent to shape the gaping hollows and the mantled crags which diversify the earth's surface. This was hinted at by Lyell in many places, although his keen eye and long experience made him qualify the conclusion by many exceptions. His scholars have had no such scruples. They have boldly and aggressively and continuously pressed the conclusion to its logical end. But they have gone further. They have iterated and reiterated the anti-Baconian aphorism that the forces of nature have always been in quality and quantity what they are now. The dictum was enunciated in all its naked absurdity by Ramsay when he presided over the geological section of the British Association, and this view has been since the inspiring philosophical creed of that most tyrannical of scientific tribunals, a Government Department—namely, the Geological Survey and its officers. Committed to preach a certain definite creed that has become the corporate geological faith of the Department, it has dominated shelves full of reports and manuals, which, from their official sanction, have almost the character of inspiration with most students. More especially has this been the case in America, where, as in this country, this bastard form of uniformity dominates official geology. Against it many of the older writers of the first rank protested vigorously, and against it I have preached myself (being only one of their scholars) all my life, and signs are multiplying that a revolution in men's opinions on the subject is not far off.

When we look at the face of a man pitted with small-pox, we refuse to believe that those hollows and scars are the normal results of human daily life instead of being the effects of a new disease which came upon mankind as a tremendous catastrophe in the last century. When we look at the moon through a telescope and examine its pitted face where the large rents and craters exceed all human experience, and when we, on the other hand, realise what peace and stillness and terrible quietude mark the face of that great cinder at this moment, we laugh at your uniformity as taught by modern geologists. It is as monstrous to suppose that any forces which have operated on the moon since human observation was turned upon it could have moulded that surface, as it is to suppose that the north wind hatches geese out of barnacles, which

was a famous deductive conclusion of the science of the dark ages. We shoot a pellet of lead at a female pheasant. It enters and injures its ovary, and the result is that that female at the next moult virtually adopts the plumage of the male, and every cell secreting colouring matter and pattern in its skin, suddenly and with a tremendous impulse and jump, proceeds at the next moult to secrete an entirely different colour and pattern. We look through our telescopes at a star which, so far as we know, has been twinkling through the ages with one unbroken light, and it suddenly bursts out into flame, a glorified conflagration in which the fire must extend for tens of thousands of miles, a gigantic catastrophe unmeasurable by any index we can get hold of here, and you tell me that the course of nature is as monotonous as the conversation of a parrot or of the never-ending imbecilities that make up the small talk of society. The thing is monstrously ridiculous. A comet goes flying through space for hundreds of years along a very oblately defined orbit, and it maintains its form as a more or less globular mass of nebulous matter. Presently it approaches the sun, and for a short time only projects into space a tail hundreds of thousands of miles in length, with a velocity and under conditions which are stupendously marvellous. Is this not an excellent object-lesson in catastrophes?

Again, let us turn to another side of the case, namely, the uniformity in the rate of change or in the intensity of the force. Because a child grows two inches a year during its early years, an orthodox geologist ought to argue that it does so in old age as well. Because the surface soil in some places grows at a rate to be measured by an inch or two in a thousand years, therefore the Carboniferous beds must have taken millions of years to deposit. As if the famous tree trunks in the Joggins Mines and others we are familiar with in the north, many feet in length and standing upright on their roots, do not make it absolutely plain that sometimes the rate of accumulation must have been exceptionally rapid. Because under certain conditions biological changes of a permanent character take a long time to develop, therefore the myriad new forms which the gardener and the breeder of animals has crowded the world with in the last two centuries, must have taken millions of years to produce.

The fact is that instead of being, as the modern geologist would argue, an exception, catastrophe is an ever-present element in the world's history; and catastrophes, or what are the same thing, phenomena new, unique and unmatched by experience, are present everywhere in the history of the past, if we will look for them and not put on Lyellian blinkers. The whole course of geology presents us with formations and with problems which we cannot match in our

present experience. Where, to take a very simple and obvious and recent case, can we in all that record match the boulder phenomena of the drift which extends over two continents, and does so independently of the contour of the country? No current phenomenon gives us even a hint as to how it was produced. Where, again, in human memory can we find among the phenomena of nature anything to compare to the great outbursts of basalt and trachyte which smother whole empires in India and South America with continuous tabular masses, and which have burst out of the ground by methods and processes apparently no longer active? Where can we match the manufacture of granite, the outflow of veins of quartz, the tossing of a huge mountain like the Rigi on end, the crumpling and reversing of the hardest crystalline rocks as if they were butter, such as we pass on our way to St Gothard, the breaking up of the chalk beds of East Anglia and of Denmark. These are mere samples of the effects of forces and of conditions which are no longer active, or rather have not been active within human memory. Potentially they may exist, no doubt, but they can no longer be seen at work. What is to be said for a President of the British Association, who was also President of the Geological Society, who in the face of such facts could deliberately lay it down that nature has always worked, not only by the same methods but with the same intensity that she does now? Uniformity, in the sense here defined, the idol of the modern geologist, underlying his teaching and the prime postulate of his philosophy, is as much a scholastic and ridiculous *à priori* prejudice as that 'all swans must be white,' that 'antipodes are impossible,' or that short-haired women and long-haired men must be exceptionally gifted.

Again, geology is a very complex science: it embodies crystallography, mineralogy, petrology, and biology. Some of its problems necessitate a training in mathematics, in mechanics, in physics, and in chemistry. They cannot be solved without such training; and yet is there any study on which so many uneducated men venture to write with the greatest assurance and impertinence? It is simply appalling to think how many men who have never been out of England lay down the most extravagant conclusions about phenomena that cannot be studied empirically at all in England. How many men whose training just enables them to map a country or to draw a section (assuredly a very elementary kind of land surveying, which few sharp boys at fifteen could not acquire), think that this entitles them to attribute the most astounding and purely imaginary qualities and properties to matter. Men who have never seen a glacier and never experimented upon ice in the laboratory, have published, and continue to publish, endless reams of unutterable rubbish about ice sheets and the movements and properties of ice, entirely based on

imagination, while others have speculated and continue to speculate upon the stupendous problems involved in the moulding of the surface of the earth and its internal skeleton with no more knowledge of dynamics than can be gathered from the "Boys' Own Book." And it is sometimes thought strange that geology is treated as a pariah by the sister sciences in which knowledge, precision, and method are deemed essential qualities of fruitful investigation.

This is especially noteworthy among official geologists on both sides of the Atlantic. It is quite shocking to think how remarkably few of them know even the elementary things necessary to their proper equipment, if they are to go beyond the province of recording facts, and to venture into that of speculations involving training in mathematics and physics. Again, what is the use of writing on science in these days, unless we know what others have written upon it? What is the use of publishing observations which have been made, and which have been published over and over again? A man who does not master the literature of his subject, and ventures to write on it as if he was the first person to face the problem, is, in my view, a scientific criminal. He ought to be absolutely tabooed. He is doing everybody a very bad service. Nay, he is dishonest. He is seated in another man's chair. Yet how many official geologists in England, and especially in America, can read any language but their own? How many of them know the absolutely essential languages—Latin, German, and French? How many of them have had any training in mathematics, and physics, and chemistry? And yet we have shelves and shelves of books paid for out of the public purse, and dealing with tremendous problems, such as the so-called glacial period, and written by men who are as guileless of equipment for such a task as children in the nursery are. These books impose upon the great mass of men, because it is thought that every official is, in a sense, an inspired person, and every book with an official binding must contain the secretions of wisdom. I know it will be said that all this is unmitigated impudence. So it is; but I know of no methods but those of impudence which can penetrate the self-satisfied assurance of a dominant school of thought, in which mutual admiration is perpetual, in which discipline requires that every man should subscribe to the Thirty-Nine Articles prescribed by his chiefs, and in which the testing of premises is deemed as unnecessary as the reading up the literature of science before venturing to write upon it. Those who care for geology as an inductive science, and who were taught its lessons by those who were inductive geologists, are disheartened beyond measure by the condition of things now prevailing in England and America, and by the elementary trifling in which so many of the dominant school, who say 'cuckoo' to each

other, indulge. There is no way of bursting our bonds but that of indulging in timely impertinence, and this can only be done by those who do not care a fig for conventional orthodoxy, and would rather be burnt with Savonarola than go on repeating worn-out shibboleths with Pope Leo or Pope Pius, with Pope Lyell or Pope Ramsay. I have now laid down some audacious impertinences. If they are false, my superstructure must be a mere building of sand, and I myself must be a more ridiculous person than even some of my orthodox friends have thought. If they be true, I may perhaps claim for my position, at least, a more lasting reputation than can be claimed for a very large part of the orthodox geology of recent years. Having laid these foundations, I shall, in the next chapter, try in some measure to justify my rashness, and grapple more concretely with the problem I set out to face, namely, to assimilate more closely than at present the terminology and methods of geology to the conclusions which accumulated experience seem to render necessary.

HENRY. H. HOWORTH.

IV

Dipeltis, a Fossil Insect?

THE figures of the genus *Dipeltis* which accompany Mr Bernard's interesting article in the last number of *Natural Science* are very suggestive. According to Mr Schuchert, they represent fossil Apodidae. To Mr Bernard himself, who adopts Mr Schuchert's view, *Dipeltis* looks remarkably like a cross between an *Apus* and a trilobite; while the first thought that occurs to an entomologist is—how wonderfully like insects these ancient Apodidae must have been. But the entomologist need not stop at this suggestion. He may venture to go further and ask—What greater reason is there for regarding *Dipeltis* as a fossil crustacean than for considering it to be a fossil insect?

The structure of *Dipeltis*, so far as it can be made out from the figures and from Mr Schuchert's description, agrees well in all essential respects with that of an insect; the three large anterior segments are easily explained as the three divisions of the thorax; this is followed by an abdomen, consisting in one species of seven, in the other of ten segments, just about the number we should expect to find; and in one species at least the abdomen bears at the extremity a pair of jointed cerci, not unlike the cerci met with in many different insects.

Unless there be some good and sufficient reason, not to be found stated in Mr Schuchert's paper, it is hard to understand why all these insect-characteristics have been entirely left out of account in discussing the zoological position of the genus. Mr Bernard has personally told me that his chief ground of objection to considering *Dipeltis* as an insect is in the presence and position of the four eyes. That would be a fatal objection, I admit, if Mr Schuchert's interpretation were proved to be correct. But as matters now stand it has no force whatever. There is no necessity to go beyond the actual facts of the case. What has to be explained is the presence, not of eyes and ocelli, but of "two faintly preserved spots" and "two small shallow pits." Assuming that the so-called head-shield of *Dipeltis* is in reality the pronotum of an insect, it requires no effort of the imagination to account satisfactorily for the presence of these pits and spots. It is not at all exceptional to meet with pits and spots and tubercles on the pronotum of insects, nor would it, perhaps, be very difficult to adduce examples in which the pits and spots approximate closely in number and position to the "eyes" of *Dipeltis diplodiscus*. Dr Woodward has described and figured a

blattid larva in the *Geological Magazine* for 1887 (decade iii. vol. iv., p. 433, pl. xii., fig. 1). In the figure of that fossil larva two spots are shown near the middle of the pronotum; and on looking through a series of existing Blattidae many species may be found in which either two spots, two pits, or two tubercles occur in a similar position. Four pits may sometimes be seen, though I have not met with any species in which one pair is placed close to the anterior margin.

But, after all, why should so much importance be attached to these eyes of *Dipeltis*, seeing that no trace of them appears in the figure of the second species.

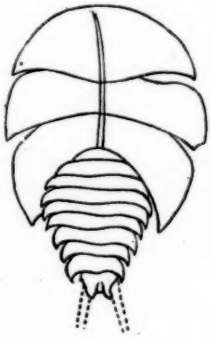

FIG. 1.

FIG. 2.

Outline figure of Carboniferous *Dipeltis* (1) compared with Photograph of Recent Larva of a Beetle (2), natural size.

So far, therefore, as Mr Bernard's objection is concerned, I think the claims of *Dipeltis* to be considered as an insect are fairly well established. And if an insect, the next question is what sort of an insect?

At first sight the figures of *Dipeltis* reminded me most of a remarkable coleopterous larva, a photograph of which I am enabled, through the kindness of my colleague, Mr Gepp, to offer for comparison (fig. 2). Nothing definite is yet known of the life history of this larva. A somewhat similar larva was figured by Westwood in his "Introduction to the Classification of Insects," and was considered by him to belong to the malacoderm series, and to the family Lycidae. In dead specimens the head is always retracted, and concealed, within the cavity of the prothorax.

That *Dipeltis* was not, however, the larva of a beetle may be inferred from the presence of the long segmented cerci in one of the species, though this fact in itself affords no absolute proof. Creatures of the cockroach kind were fairly abundant in Carboniferous times,

and it is amongst those that the true relations of *Dipeltis* will probably have to be sought. The abdomen of *Dipeltis* is narrower in proportion to the thorax than is the case with existing cockroaches, but a similar condition may be seen in the figure of the fossil larva referred to above; and if this figure be compared with the figures of *Dipeltis* one cannot help being struck by the agreement in general shape, and in the number and relative size of the parts.

In the facts, apart from the speculations, which are disclosed in Mr Schuchert's account of *Dipeltis*, I find nothing that is inconsistent with the view I have ventured to put forward in this paper; while it is interesting to note that "these animal remains are found in [Lower Coal Measure] nodules associated with *Prestwichia*, *Eurypterus*, malacostracous, schizopod and decapod crustaceans, arachnids, insects, *Aviculopecten* and *Solenomya*, and numerous ferns."

Dr Packard, who established the genus *Dipeltis*, is a distinguished entomologist, and may very well be trusted to recognise a fossil insect when he sees one. But so far as I am aware he has not yet expressed an opinion upon the more perfect remains that have come to light since he first described the genus from a mere fragment of a specimen. I shall be anxious to know what that opinion is, and shall be surprised should it not support my own view of the present question.

C. J. GAHAN.

[I may as well at once confess that, until my attention was called to them by my entomological friends, I was quite ignorant of the existence of insects, either adult or larval, showing the habit of *Apus* so nearly as does *Dipeltis*. I venture to submit that very few zoologists, who are not specialists in entomology and acquainted with these particular forms, would recognise anything specially Hexapodan in *Dipeltis*, which does not even show the ordinary insect segmentation with its antennae-bearing head. I do not, therefore, think that much blame attaches either to Mr Schuchert or myself that we failed to detect the insectiform characters of *Dipeltis*, hidden away as they are under the guise of *Apus*.

Leaving, however, this unimportant personal question, I think it may be easily gathered from my remarks on *Dipeltis*, in the paper referred to by Mr Gahan, that the Apodidae would not lose much by giving it up. Its claims to belong to that ancient family required a strong pair of pincers to bring it into line. This is, I think, the chief argument in favour of Mr Gahan's claim that *Dipeltis* belongs to the entomologists: it fits easily into their system, thanks to the existence of certain larval forms, but only with great difficulty into the Apodidan. But for this difficulty, which is, by the way, only revealed by careful analysis, we should have to wait for further evidence before coming to any definite decision as to its affinities.—H. M. B.]

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V

Reproductive Divergence not a Factor in the Evolution of New Species

AS it is of fundamental importance to know whether a species can split up into two or more new species in one and the same locality without the aid of any kind of local separation, or whether isolation of some kind is a *conditio sine qua non*, I ask leave to reply to Mr Vernon's article in the last issue of *Natural Science*, in which he tries again to demonstrate mathematically the correctness of his theory of Reproductive Divergence. For the sake of argument and brevity I accept Mr Vernon's figures as being correct, as I did in *Natural Science*, Vol. XI., p. 317. Then we have as the result of the chance intermarrying of 900 parents under ordinary circumstances the following number of offspring (of each sex):

I.	*a.* Parents,	300	short,	300	medium,	300	tall = 900.
	b. Offspring,	293	„	314	„	293	„ = 900.

If now the fertility of parents of the same size is assumed to be higher than that of parents of different size, the figures are according to Mr Vernon, as follows:

II.	*a.* Parents,	300	short,	300	medium,	300	tall = 900.
	b. Offspring,	300·6	„	318·8	„	300·6	„ = 920.

By a comparison of the offspring II. *b.* with the offspring I. *b.*, which, however, have nothing to do with one another, the individuals under I. being physiologically different from those under II., Mr Vernon comes to the conclusion that "short and tall individuals have increased by 2·59 per cent., but the intermediate ones only by 1·52 per cent." It is obvious that the figures under II. *b.* can only be compared with those under I. *b.* after the total of II. *b.*, which amounts to 920, has been reduced to 900. But that is a mere oversight in the manipulation of the figures and of no great importance, though the figures will, after the reduction, be very different from what they are now. The grave error in Mr Vernon's interpretation of the figures lies in the sentence following the one quoted before: "There is thus a gain of about 1·07 per cent. in favour of the extreme individuals over the intermediate

ones, or supposing the original 300 medium individuals had again been produced, there would now be 303·2 instead of 300 tall and short ones." I have to reply:

(1) That, as the individuals under II. *b.* are not the offspring of those under I. *b.*, but of the individuals II. *a.*, the number of tall and short offspring II. *b.* is smaller than the number of their tall and short parents under II. *a.* after the reduction to 900.

(2) That against every 300 medium individuals three are under II. *b.* only 282·9 short and talls ones.

(3) That Mr Vernon changes the 1·07 per cent., which, by the assumption of greater fertility of parents of the same size, the short and tall offspring II. *b.* gain more than the medium-sized offspring II. *b.* over the respective numbers of the offspring I. *b.*, into an excess of the number of small and tall offspring II. *b.*, over the number of medium offspring II. *b.*, and hence comes to the astonishing conclusion that the 900 parents II. *a.* produce 303·2 short and 303·2 tall offspring against 300 medium ones.

(4) That the before-mentioned gain of 1·07 per cent. on the part of the short and tall offspring II. *b.*, means nothing else than that the curve representing the offspring II. *b.* is not so convex as that representing the individuals I. *b.*

Apart from mathematical details it is evident from Mr Vernon's figures on p. 405, Vol. XI., that the degree of fertility of the three sets of parents of equal size can have no influence upon the proportional numbers of tall, medium, and short offspring, as long as the three sets are equally fertile. The difference in the numbers of the three sets of offspring is entirely dependent on the results of the intermarrying of parents of different size. From the lower figures on p. 405, Vol. XI., we see that there will always be more medium than tall and short offspring produced by these marriages, however much less productive these marriages may be than those of parents of the same size. The number of medium offspring produced by the intermarrying of short with tall parents has certainly decreased by the assumption of lesser fertility of such parents, but so have the numbers of short and tall offspring.

A consideration of another kind will perhaps more obviously show that there is an error concealed in Mr Vernon's mathematical demonstration. The above 920 offspring II. *b.* are produced by 300 short, 300 medium, and 300 tall parents; according to Mr Vernon's own calculations the set of 300 short and the set of 300 tall parents have 300 offspring each, while the 300 medium parents produce 320 offspring. The medium individuals are, therefore, according to Mr Vernon's figures, the more favoured as regards fertility and hence must necessarily gradually replace the tall and small ones, provided (as is assumed by Mr Vernon) that the three

sets of individuals have no advantage over each other in respect to other factors.

But the best test of the fallaciousness of the hypothesis we shall have, when we accept Reproductive Divergence as a true factor, and apply this factor again to the two races into which the original species has developed by means of Reproductive Divergence. Let us then suppose that the 900 individuals are split up into a race of 450 short and a race of 450 tall individuals. The mean of the short race is "supposed" by Mr Vernon to be 65·8 inches—why it must be 65·8 inches I do not know. As "the members of this group deviate in either direction (spaced type mine) from the average size [65·8 inches] in the same proportion as the members of the original group did, it follows that 10 per cent. of this group will be below 62·2 inches in height." Certainly; but the average size being 65·8 inches there will also be 10 per cent. above 69·4 inches in height! The members of the tall race will, of course, deviate in a similar manner; the medium size of this race being 70·6 inches there will be 10 per cent. of the individuals below 67 inches. Where is the gap between the two races? Let us, however, suppose there be a gap between them and apply the principle of Reproductive Divergence to the two now separate races. It is self-evident that the short race will split up into a still shorter one and into a taller one, and that the tall race will develop into a less tall one and a more tall one, and so on. Will not the gap which we assumed to exist between the above two races be filled up now? Reproductive Divergence itself will mend the gap which it is claimed to tear into the evenly convex curve of a species.

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VI

The Structure and Habits of the Plesiosauria.[1]

SINCE the discovery of the Plesiosauria and the determination of their characters, which are so completely different from those of all the existing reptiles, palaeontologists engaged in studying them have often attempted to reconstruct them in their outward form and natural surroundings, or, so to speak, to bring them back to life. Conybeare, the describer of *Plesiosaurus dolichodeirus*, which was first found in the form of complete skeletons, quite rightly determined its position among the reptiles, its aquatic habits, and its character as a beast of prey. The diagrammatic sketch of the skeleton given by him also avoids an error which was afterwards repeated and continually exaggerated, since it represents the neck not bent like that of a swan, but only gently curved as a whole. Nevertheless, he states in the text, that he also ascribes to the neck great flexibility and the power of snatching prey, and recognises in the first character a compensation for the want of a large mouth, with powerful jaws and strong teeth, such as the Ichthyosauria possess. He represented the Plesiosaurus as a creature lying in wait for prey in the sea-weed in the shallower parts of the sea near the shore, the body under the water, the nostrils projecting from it for breathing. While a more sluggish mode of life was thus postulated, other workers ascribed to the Plesiosauria a quick restless movement on the surface of the sea, either in the neighbourhood of the coast between the rocks or in the open sea, rapidly darting the long flexible neck and the small head to obtain food. Restorations which illustrate this view, accepted, among others, by T. C. Winkler, Hutchinson, and Koken, are to be found in the works of the two first-mentioned authors, and are based on the assumption of an easily moveable swan-like neck, sharply separated from the barrel-shaped body. This idea was, indeed, carried to such an extreme that Plesiosaurus was compared to a snake drawn through the shell of a tortoise, giving rise also to the idea of a head standing out at an angle from the vertebral column of the neck like that of a bird.

From a possibly more careful examination of the separate parts

[1] Translation of the final chapter of a memoir on the Plesiosauria of the South German Lias Formation in *Abhandl. k. preuss. Akad. Wiss., Berlin*, 1895, pp. 75-80. Revised by the Author, 1897. The illustration (Plate III.) kindly lent by the Berlin Academy.

of the vertebral column of the neck in comparison with that of the trunk, I cannot regard as proved either the outer form just mentioned or the easy power of motion of the neck.

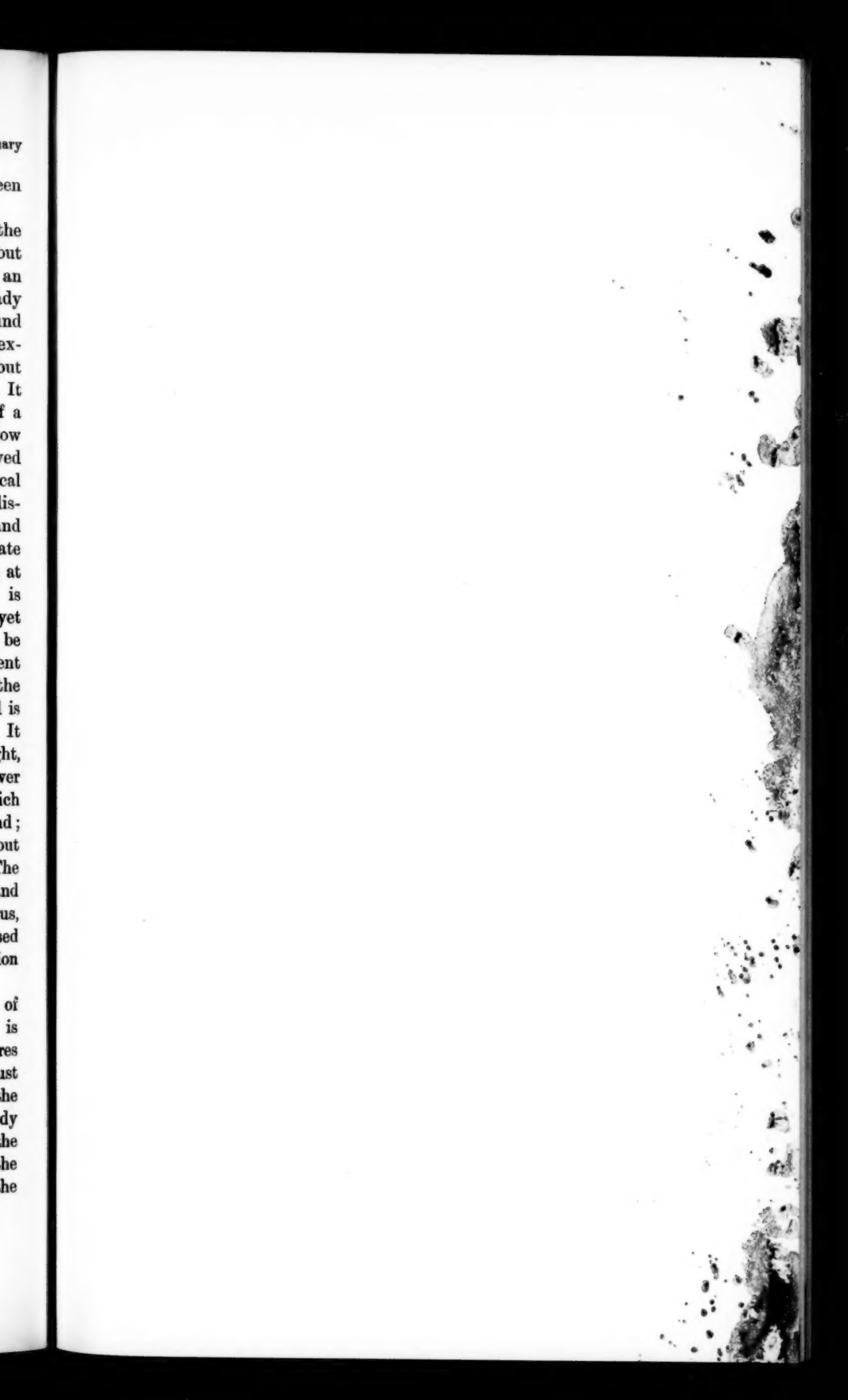
If the head had been fixed at an angle to the vertebral column, the occipital condyle must have been more or less sharply turned downwards, which is not the case, as will be seen from a glance at the figures of the skull of *Plesiosaurus rostratus* and *P. hawkinsi* given by R. Owen. Moreover, the skulls always lie in the same stratum as the vertebrae, and almost always in direct continuation of them. The neck-vertebrae increase very gradually in all dimensions from the head towards the trunk; the neural spines which become higher and higher backwards, as also the strong cervical ribs, indicate a well-developed muscular covering which was capable of moving the neck as a whole, but not its separate parts one on another, such as is rendered possible in the neck of the bird by the saddle-shaped articulations and the almost uniform size of the vertebrae from the first to the last. So highly developed a power of motion is rendered improbable by the almost flat articular ends of the neck-vertebrae of most species, as also by the high neural spines, and especially by the double-headed cervical ribs, which overlap each other like those of the crocodiles. The Plesiosauria could indeed move their neck powerfully and quickly in all directions, but more like an elastic rod as a whole, not in **S**-shaped bends like the neck of a bird. This conclusion, based on the structure of the neck-vertebrae, also receives important confirmation from the arrangement of the more or less completely preserved skeletons. The drawings of complete skeletons in the works of Conybeare, R. Owen, T. C. Winkler, and Sollas show that the large majority of individuals are preserved with the neck quite straight and lying in the long axis of the animal, or at most only slightly curved. In the latter case the curvature is met with only in the front half, while the hinder portion extends in a straight line. This is also the case in the skeletons in which the maximum curvature is attained, namely those of *Plesiosaurus macrocephalus* and *P. brachycephalus*, R. Owen, both with a remarkably large head and short neck; but even here the curvature of the front portion is very far from being a semicircle.

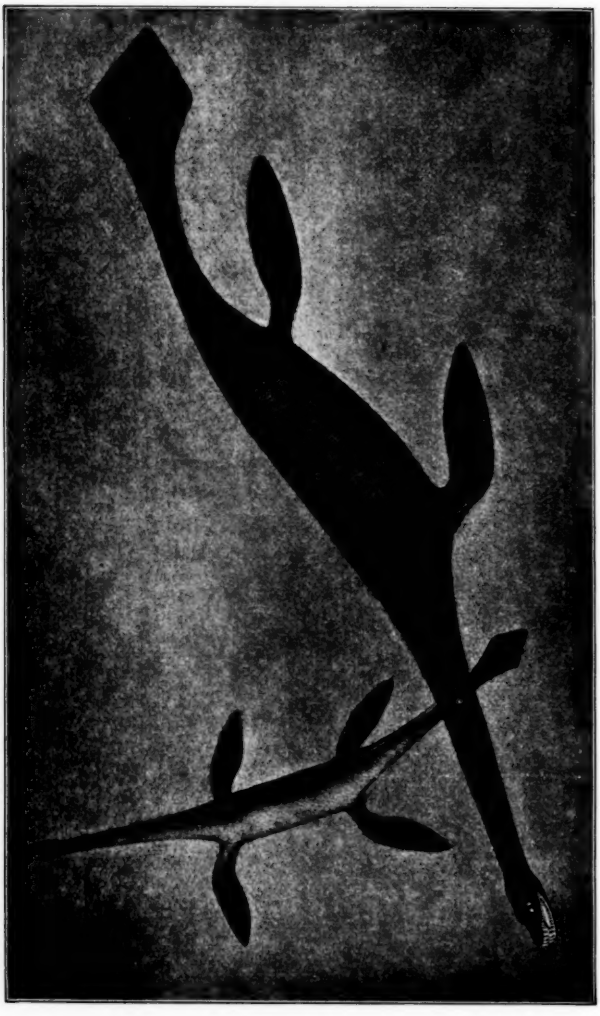
It is, moreover, to be remarked that the neck was not sharply separated from the body, but gradually passed into the trunk, because the vertebral centra are similar in size, and the neural arches correspond exactly in height and breadth with those of the last neck-vertebrae. This proves also that the long bands of muscle which extended along the upper part of the neck-vertebrae passed into the front of the trunk without an increase in size; and thus the hinder part of the neck-region must have been as thick as the front

part of the trunk. No sharp line of demarcation could have been observed externally between the neck and the trunk.

In the representation of the trunk and the limbs given in the figures already mentioned, there is scarcely anything to alter; but the *Plesiosaurus guilelmi imperatoris* now described, permits an important addition to the end of the body. Sollas has already observed remains of the skin in *Plesiosaurus conybearei*. He found it lying upon a part of the dorsal vertebrae and their ribs, and extending as a narrow band over the ends of the neural arches; but the specimen investigated by him showed no trace of a tail fin. It is to be remarked that R. Owen inferred the original presence of a tail fin from the form of the caudal vertebrae, and this idea is now confirmed. It is determined from the parts of the skin preserved that the Plesiosauria, like the Ichthyosauria, possessed a vertical dermal fin at the end of the body. More nearly complete discoveries in the future will perhaps definitely determine its size and shape. The accompanying illustration of the Swabian species (Plate III.) represents it as it seems to me probable from the fragments at my disposal. If the parts go together, as I suppose, its length is approximately given; but whether the height is correct cannot yet be decided. To justify the rhombic form given to it, it may be pointed out that among all the reptiles and amphibia of the present day which possess a vertical expansion of skin at the end of the body, such as crocodiles and salamanders, it ends in a point, and is never split into two lobes like the ordinary tail fin of a fish. It follows from this that, if the vertebral column remains quite straight, the fin may thus be divided equally between the upper and lower side. It is different among the fishes and Ichthyosauria, in which the vertebral column is bent upwards or downwards at the end; and among the teleostean fishes it is not made of a single piece, but results from the fusion of the tail fin and the posterior anal fin. The outwardly homocercal caudal fin is also inwardly heterocercal, and its division into two lobes is in direct relation to this. Plesiosaurus, however, with its absolutely straight tail can thus have possessed only a pointed, undivided, dermal fin, and hence the restoration given here.

This restoration shows yet another difference from those of earlier authors. If the determination of the facts given above is correct we can no longer represent the Plesiosauria as creatures living on the surface of the sea like swimming birds, but they must have lived, like the Ichthyosauria, beneath the surface of the water. For such a mode of life a pointed anterior end of the body is important, and this is here brought about by the elongation of the neck and diminution of the head, while on the other hand the Ichthyosauria have entirely lost the neck, and therefore extended the





RESTORATION OF PLESIOSAURUS
By Professor W. DAMES

MICH.

head in a long porpoise-like snout. The principal power of locomotion in the Plesiosauria resided in the limbs, as is indeed evident from their remarkable length in proportion to the trunk. The fore and hind extremities were also nearly of equal size. That the latter were as powerful for locomotion as the former is shown by the structure of the pelvis, which, with its great spreading pubis and ischia, afforded space for the attachment of large muscles, just as the coracoids in front provided for the fore limbs. Here also there is an important difference from the Ichthyosauria, whose pelvic elements are reduced to delicate rod-shaped bones, corresponding with the small hind limbs. Further, while the Ichthyosauria relied on the large downwardly heterocercal tail fin for a good part of their locomotion, as recent discoveries in Swabia have shown, the tail fin of the Plesiosauria was less suited for that purpose on account of its smaller expansion. The frequently enlarged neural arches of the last caudal vertebrae indicate that bands of muscle extended into the fin, so that it would admirably serve as a balancing and steering apparatus.

The accompanying drawing (Plate III.) represents two Plesiosauri as they may have appeared according to the conclusions here advanced in reference to their shape and habits. The one individual is swimming below the surface of the sea in search of prey, the other is rising to the surface to breathe. They are represented of about one-twentieth the natural size.

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SOME NEW BOOKS

Wild Traits in Tame Animals

Wild Traits in Tame Animals: being some familiar studies in Evolution. By Louis Robinson, M.D. 8vo. pp. xii. and 329, illustrated. Edinburgh and London: Blackwood & Sons, 1897. Price, 10s. 6d. nett.

Darwin made a great point of the insight afforded into the habits of their bold ancestors by many traits exhibited by our domestic animals, such as the scratching of tree-stems by cats to clean or sharpen their claws, the turning round of a dog before settling to slumber on the hearthrug, and the pawing of snow by horses. And it has been the object of the author of the work before us to hunt further instances of such ancestral traits, both as regards bodily and mental habits.

The author has a pleasant and attractive way of expressing himself, so that he is likely to draw a much larger circle of readers than many writers of popular science. And some of his inductions are well founded and of considerable interest. For instance, he tells us that from the quiet unaggressive way it takes its food, it may be inferred that a cat is naturally a solitary animal, whereas, on the other hand, a dog must be a gregarious creature, forced to get its food by snatching it as quickly as possible in order to prevent its being seized by a rival. And, although it is perfectly obvious, we do not recollect to have seen it expressed elsewhere that "recognition-marks," like the white under surface of the tails of fallow deer and rabbits are actually injurious to the individual, and advantageous only to the tribe as a whole.

If all the inductions and theories of the author were as well founded as the above, there would be little or no scope for destructive criticism. But there are some, which, to put it mildly, are more than doubtful. As an example may be cited the supposed mimicry of snakes by cats, both as regards coloration and hissing. This appears to be based on a certain resemblance, so far as coloration is concerned, between an ocelot when coiled up and a boa. And it is argued that in early times all cats were small and coloured somewhat like the ocelot. There are many objections to this startling generalisation, but it will suffice to say that the ocelot type is almost certainly not the original feline coloration. Again, as regards the ancestry of our European domestic cats, it is seriously argued that the tabby, grey, and tortoise-shell-sandy breeds are derived from as many unknown extinct species. This obviously implies a want of acquaintance with what constitutes a species, and the great colour variability inherent in domesticated animals. If such a theory was admitted, what would become of Darwin's conclusions as to the common ancestry of our domesticated pigeons? But the most astounding suggestion of all is that the tortoise-shell-sandy breed is derived from a wild species of which the females inhabited scrub, and the males open sandy districts. Is there any evidence of a wild animal of any kind in which the two sexes have such dissimilar habitats?

In his introduction, the author is somewhat severe as to the treatment accorded to the amateur naturalist by the specialist; and he seems to fear that the former class is in danger of extinction by the latter. If amateurs will indulge in wild theories, there is in truth some likelihood of their not receiving the attention they undoubtedly merit at the hands of their professional brethren. For both there is plenty of good work remaining to be done, the amateur, as a rule, being the one by whom our knowledge of animal habits is advanced. Before, however, he proceeds to theorise it is essential that he should have a fair grasp of the leading principles of zoology; and if that be attained, he has the chance of advancing some brilliant hypothesis, which, from over caution, the specialist would never dare to propound.

If the reader be sufficiently advanced as not to take for gospel all that he finds set before him, there is much worthy of his best attention in this latest contribution to a very interesting subject.

Verworn's Physiology

Allgemeine Physiologie. Ein Grundriss der Lehre vom Leben. By Max Verworn, a.o. Professor der Physiologie an der medicinischen Facultät der Universität Jena. Second Edition, with 285 illustrations. 8vo, pp. viii. 606. Jena: Gustav Fischer, 1897. Price, 15 marks.

The first edition of this work has already been fully reviewed in these pages; the second, therefore, calls for no more than a brief notice of new features. The addition of seventeen new illustrations, and the incorporation of some new matter, have increased the bulk of the volume, but there is little material alteration in the text, except that one section of the first chapter has been rewritten and much shortened, and now bears the heading "Psychomonismus," instead of the original "Psychologische Methodik."

The figures are, on the whole, very much better executed than those in the first edition; some which were unimportant have been omitted, some others improved upon (see the figure of *Orbitolites* on p. 98), and many new ones added which are a distinct gain to the work. Among the latter, the full-page illustrations of *Rhizoplasma kaiseri* (p. 289) and of *Hylopus* (p. 333) are worthy of special mention. We note that the elaborate "Quadrille des centres," from Boveri, is now conspicuous by its absence. One feature in the new edition which we do not regard as an improvement is the use of the same figure to illustrate different points. These repetitions occur with irritating frequency; for instance, a very commonplace *Amoeba* appears no less than three times, on pages 88, 97 and 172; a dividing ovum (after Boveri) on pages 72 and 199; while the familiar *Paramoecium* and *Spirogyra* are reproduced twice respectively. We fail to see the necessity for this system of "carrying forward"; a reference to a previous page would have met the wants of a student who could not bear in mind for eight pages the details of structure of an amoeba, and lazier readers should not receive consideration.

Nemertine Worms

Traité de Zoologie. Publié sous la direction de Raphael Blanchard. Fasc. XI. Némertiens, par Louis Joubin, professeur à l'Université de Rennes. 8vo, pp. 59, and 53 figg. in text, 18 of them coloured. Paris: Rueff & Cie., 1897. Price, 2 fr.

Professor Joubin has given us a concise, well-arranged, and very readable account of the Nemertines. After giving a short sketch of

their biology and general anatomy, he proceeds to give a comparative review of the different organs, or systems of organs, in turn, followed by a very clear account of the three types of development, and concluding with a few pages on the systematic arrangement of the group and its relation to other groups, especially to the Turbellaria. It is, however, to be regretted that, in his anatomical part, he employs the term "coelome" to designate what has no more morphological than it has etymological right to the name, and without even attempting to justify his use of it, or perceiving that it is necessary so to do. There are also a few apparent discrepancies in his descriptions, arising from the fact that, while he admits Bürger's classification to be the more natural, and adopts it finally in the systematic part, he uses Hubrecht's names for the "sections" throughout the anatomical part, and does not seem always to remember that they do not connote the same thing as the names used by Bürger. It would have been clearer, as well as more consistent, to have used throughout the same classification.

Most of the figures are diagrammatic, and it is, we think, unfortunate that so many of these are of the composite type. Not only are such figures misleading, since they represent structures side by side which do not so occur in nature (as, for instance, in the section of the epidermis on p. 11), but also it is very difficult to make them strictly accurate. Thus, by representing in one diagram the proboscis of a nemertine invaginated on one side and evaginated on the other, the impression is produced that evagination is due rather to the drawing back of the proboscis-sheath than to the pushing out of the walls of the proboscis itself, since this organ is no further forward in the one case than in the other, while its junction with its sheath is further back on the evaginated side. Again, one would gather from Fig. 18 the erroneous impression that the vascular system of *Carinella* ended posteriorly in the same way as that of *Carinoma*, also that all species of *Carinoma* had the two blindly-ending longitudinal vessels anteriorly that are there represented; and from Fig. 21, since nothing to the contrary is stated in the text, that the excretory system of *Amphiporus*, and of all species of the genus, communicates with the exterior by five pairs of ducts, whereas Oudemans, from whom, as we are told, the diagram is taken, not only expressly states that in some species there are but two ducts, but also that in the particular species to which the diagram refers the number is not even the same on the two sides of the body. Prof. Joubin, as it happens, represents on the right side what Oudemans made out to be the arrangement of the excretory system on the left.

It would also have been an advantage to the reader if the same reference letters had been used for the same structure in all the figures, and if the text and figures had harmonised better with one another. One does not know, for instance, whether one is to believe the text or the figure with regard to the position of the deep glands of the Schizonemertines (p. 12 and Fig. 7B), or with regard to the position of the nerve-cord in *Cephalothrix* (p. 29 and Fig. 7A); and it is also somewhat confusing to find that in the description of the characteristic features of the proboscis of the Unarmed nemertines (p. 16), the figure of a papilla in an Armed one is referred to, while it

is nowhere mentioned that, in the latter group, papillae occur at all. Fortunately, however, a bibliographical index is given, so that it is not necessary to remain long in doubt as to the facts.

Cyclops

Bulletin of the Illinois State Laboratory of Natural History, Urbana, Illinois. Volume v. Article ii. A contribution to a knowledge of North American Fresh-water Cyclopidae. By Ernest B. Forbes, B.S., 1897.

Anales del Museo Nacional de Buenos Aires. Tomo v., pp. 331-332. Sur quelques Entomostracé's d'eau douce des environs de Buenos Aires. Par Jules Richard, 1897 (Apareció el 8 de Junio).

Fifteenth Annual Report of the Fishery Board for Scotland, Part iii. The Invertebrate Fauna of the inland waters of Scotland. Part vii. By Thomas Scott, F.L.S., Mem. Soc. Zool. de France. Plate ix. 1897.

Mr Ernest B. Forbes has utilised an academical essay to adjust the synonymy of the fresh-water Cyclopidae of North America. Unfortunately he has to admit that his revision of the group is still incomplete. He institutes two new subgenera. Some people like subgenera; others regard them as scientific misfortunes. In compensation Mr Forbes cancels several species too hastily introduced as new by American writers. These results have evidently not been won without much laborious and conscientious comparison of specimens. Whether all Mr Forbes' conclusions are valid it would be premature to decide. At the outset he lays stress on the fact that in the Cyclopidae, "of all the specific characters, the most valuable are those derivable from the *receptaculum seminis*." Then, on the ground of certain microscopic characters, he upholds the *Cyclops edax* of his instructor, Dr S. A. Forbes, as a distinct species from *Cyclops leuckarti* Claus, although he himself says "the structure of the *receptaculum seminis* is identical in the two species, so far as I can tell." But of *Cyclops edax* he makes *Cyclops annulatus* Wierzejski a synomyn. Rather opportunely, therefore, Dr Jules Richard has just published a figure of the *receptaculum seminis* in *Cyclops annulatus*, relying on it as a character distinguishing that species from the neighbouring *Cyclops leuckarti*. But if the *r. s.* in *C. edax* agrees with, and the *r. s.* in *C. annulatus* differs from, that in *C. leuckarti*, it would follow that *C. edax* and *C. annulatus*, cannot be identical, or else that the *r. s.* does not deserve to have much stress laid upon it.

The most notable feature separating *C. edax* from *C. leuckarti* is afforded by the hyaline plate of the last joint in the first antennae. In the latter species this has a characteristic notch, not present in the former. Mr Thomas Scott, who has within the last few weeks added *C. leuckarti* to the known fauna of Scotland, makes a reference to this notch, but speaks of it as a feature of adult specimens, though without definitely stating that it is not found in younger stages. The coincidence of three writers in different parts of the world independently busying themselves with the characters of this little species, of the length of one millimetre, must give an impression that in this age the fauna of the globe is being very carefully scrutinised.

A System of Bacteria

System der Bakterien: Handbuch der Morphologie, Entwickelungsgeschichte und Systematik der Bakterien. By Dr W. Migula. Band I., Allgemeiner Teil. 8vo, pp. viii+368, with 6 plates. Jena: Gustav Fischer, 1897. Price, 12 marks.

Of the making of books on bacteriology there is no end, yet as very many of these books are scarcely worth the paper they are written on, any really solid contribution to bacteriological literature ought to meet with due appreciation. Dr Migula's first volume gives ample promise of a very useful work, and contains a general survey of the classification, morphology, and development of schizomycetes. It is much more than a text-book; it is full, not only of well-digested information, but of original facts and suggestive hypotheses. The first section is devoted to a historical review of the classification, which concludes with the author's own scheme, a later modification of that already published by him in Engler and Prantl's work. The second and most interesting section deals with morphology and life-history, and the chapters treating of controversial questions, such as spore-formation, pleomorphism, etc., are worth reading, not so much for their weight as evidence as for their suggestiveness. Argument is perhaps hardly Dr Migula's strong point, and we think he is too apt to put forward as facts statements which, while unsupported as they often are by the quotation of a single instance, we are bound to regard as assumptions. These very statements, however, often contain ideas well worth working out to their logical conclusion. The question of pleomorphism is more or less begged, but we gather that Dr Migula is not so strong an opponent of the theory as we were led to believe by his earlier writings. The pros and cons are at any rate retailed with admirable fairness. The third section is concerned with the discussion of chiefly physiological characters and various biological criteria of species.

On the whole, the author and his readers are to be congratulated on this first instalment of the work. The plates are very good, and the bibliography at the end of each chapter exhaustive and exact.

Another Work on Bacteria

Vorlesungen über Bakterien. By Dr Alfred Fischer, a. o. Professor der Botanik in Leipzig. 8vo, pp. viii+186, with 29 figs. Jena: Gustav Fischer, 1897. Price, 4 marks.

Dr Alfred Fischer's book does not need the apology he makes for its publication in his preface; a book on non-pathogenic bacteria, or, more strictly, one from which medical bacteriology is entirely excluded, would supply a real want, and the work in question might have answered this purpose if the subject had been given fuller treatment in a less popular form. It is very good as far as it goes, but, like Oliver Twist, we 'ask for more,' and it is to be hoped that in a second edition, which is sure to be called for, Dr Fischer will remedy this defect, and expand his modest volume into a work at once comprehensive and detailed enough to satisfy the demands of the researcher as well as of the student. The author writes very clearly, and is to be congratulated on the exactness of his terminology. As an example of his accuracy in description, he confines his use of the word 'Faden' to strictly filamentous forms, whereas nine out of ten writers habitually apply this term—or its English bacteriological equivalent 'filament'—

to such distinct structures as a true sheathed or unsheathed filament, a branched zoögloea, and a moniliform series of cocci. To make confusion worse confounded in this way is inexcusable, and we hope Dr Fischer's better example may find followers. The captious critic might perhaps suggest that, in using the word 'zusammengesetzteste' (p. 3), the author, without malice prepense, casts a slur on the German language which it does not deserve; it is certainly rich enough to supply him with a less barbarous superlative. The book deals almost exclusively with such bacteria as are concerned in metabolism, fermentation, nitrification, and various physical and industrial processes; it contains brief chapters on morphology, classification, distribution, habitat, and conditions of life, the general principles of nutrition and culture, respiration, and effects produced by chemical and physical agencies, with a more detailed account of the relation of micro-organisms to nitrogen and carbonic acid, and a short mention of bacterial diseases affecting plants and animals. The figures in the text are excellent; many are new, at any rate to text-books, which are too often illustrated only by miserable reproductions from the most antiquated sources. Altogether the book is one which may be read with both pleasure and profit in its present form, but which merits and might command a wider circle of readers.

Botany in the Field

Open-Air Studies in Botany: Sketches of British Wild Flowers in their Homes. By R. Lloyd Praeger. 8vo, pp. xiii + 266, with frontispiece, 6 plates and 68 illustrations in the text. London: C. Griffin & Co., 1897. Price, 7s. 6d.

"Come forth into the light of things,
Let nature be your teacher."

The quotation from Wordsworth which appears in the title page of this book is an appropriate text for the matter contained in its eleven chapters. In a series of brightly written descriptive accounts of familiar scenes in our own islands, Mr Praeger contrives both to interest the reader and tell him a good deal worth knowing about plants. "We stand," he says, "in fancy, out in the open country, with the wild flowers at our feet, the hum of insects and the rustling of the wind in our ears, and the blue sky overhead, and we use these powers of observation that have been given to us. Thus only can we hope to comprehend the life of a plant, or of a plant-community, and appreciate the conditions under which each species lives, and the adaptations by which each is able to maintain its position in the plant-world, and fulfil its proper functions." It is not a text-book, and with students working merely for examination will not find favour; but to nature-lovers in general, and wild-plant lovers in particular, "Open Air Studies" will be a very welcome and a very healthy companion and guide. Being resident in Ireland, the author has taken most of his scenes "from the meadows and mountains, streams and seashores of that fair land," and the addition of place and date beneath the title of each chapter gives a circumstantial appearance, and adds a spice of interest to what follows. But as the spots selected are not exceptional in their vegetation, and familiar flowers not rarities are described, the majority of the scenes might be equally well located in similar places in any part of the British Isles. They are as follows:—A

Daisy-Starred Pasture, Under the Hawthorns, By the River, Along the Shingle, A Fragrant Hedgerow, A Connemara Bog, Where the Samphire Grows, A Flowery Meadow, Among the Corn—a Study in Weeds, In the Home of the Alpines, and a City Rubbish Heap. We get an idea of the characteristic vegetation of each place, and the individual plants are made the pegs on which to hang facts of elementary botany; and, on the whole, the science is not by any means so disjointed as might be imagined from this method of treatment. He who has visited these or similar scenes with Mr Praeger as his guide, will have got knowledge enough to enable him easily to acquire more, and thereby to satisfy a craving which we are quite sure he will feel.

Our notice would be incomplete without some reference to the illustrations. Those in the text, the work of Miss Rosamund Praeger, are useful, though by no means ambitious; but the plates which are reproductions of photographs, taken by Mr R. Welch, are very nice, the frontispiece, "When Daises Pied," the group of Reed-mace on the Boyne (Plate I.), and a Cavan Meadow (Plate VI.), showing Purple Loosestrife and Meadow-sweet, call for special mention, and so do his other four.

British Wild Flowers

Familiar Wild Flowers. Figured and described by F. Edward Hulme, F.L.S. Cheap Edition. 5 vols. 8vo, with 40 coloured plates in each. London: Cassell & Co., 1897. Price, 3s. 6d. each volume.

We are glad that Messrs Cassell have issued Mr Hulme's sketches and chats on familiar wild flowers in such form as will bring them within reach of many flower lovers. Our 'wayside weeds' are so beautiful, and their study is such a healthy, restful recreation, that any genuine effort to bring them nearer to us, and to help the would-be student, is always welcome. But we rarely find a teacher who can give us a pleasing and life-like picture as well as tell us, without being prosy or technical, something worth knowing about our plants. Mr Hulme is not a botanist; much of what he says is irrelevant gossip, and might well have been replaced by useful facts. But he does make us feel what pretty and interesting objects are to be found in the country lanes, the fields, and the woods. He is not by any means systematic. Beginning with the field convolvulus, we come next to the field rose, then the meadow crane's bill, then silverweed, apple, borage, and so on, higgledy-piggledy all through the five volumes. The only sop to the scientific is a brief summary at the beginning of each volume, while a short botanical diagnosis, such as we find in the "Student's Flora," is given for each plant contained therein. It would have been quite easy to arrange the two hundred odd species in some systematic order, and such arrangement would have pleased everybody. The book would then have been a useful companion to any of our smaller British floras, and thereby a great help to the more serious student, while even the most popular-minded person would, we think, prefer to find his roses or his buttercups associated in the same volume. We would also suggest that some of the plates are getting very worn; most of them, in fact, look best by candle light, but some, like that of the periwinkle in the first volume, had better been left out altogether.

As regards the text accompanying the plates, we see from it at once that the author has an eye for the general form and habit of the plant,

which he describes in simple language, and tells where it grows and thrives best, and when we may find it in flower. We learn something too of its history, and what Gerarde, Ray, and many another old botanist (whose name and work is often unknown to the modern student, for whom botany was founded by Sachs) thought about it. The popular names afford scope for a good deal of speculation; in many cases where they originate from the economic or medicinal use to which the plant was put in olden times, or bear some relation to the shape of the flower or the leaves, the derivation becomes sufficiently obvious. But often the writer walks on very thin ice; and we are bound to admit that a popular name was vaguely applied to several plants whose appearance for instance coincided with some well-marked recurring event, such as the arrival of the cuckoo in the case of the numerous 'cuckoo flowers.' We might say more about this handy little series of volumes, but we have said enough to recommend them to those who want an inexpensive, popular and, within limits, useful account of British plants.

A French Text-book of Botany

Traité de Botanique. By L. Courchet, professeur d'histoire naturelle à l'école supérieure de pharmacie de Montpellier. 2 vols. 8vo, pp. viii + 1320, with 514 figs. Paris: Baillière, 1897. Price, 18 francs.

Prof. Courchet has added one more to the long list of text-books, and has produced a book no worse than many other treatises in botany, and better than some. According to his preface it embodies the course of lessons which, for seven years past, he has put before his students at Montpellier, and which he tells us has been attended with happy results. We hope, for the students' sake, that the course included well-arranged practical work; as an accompaniment to such the two volumes might be made useful, but taken alone would prove very tedious reading, for the attractiveness supplied by the illustrations, which are fairly numerous, is counterbalanced by repellent-looking tables which make one feel giddy and thankful that examination days are past. The most useful portion of the book is the first part on general botany—an introduction to the morphology and physiology of plants, with a chapter on the principles of classification. The whole of this occupies only 176 pages, and for the size of the book should have been much expanded. The least satisfactory portion is that dealing with physiology. The account of the modification of the various members is also extremely meagre. The second part, 'Special Botany,' is divided into two—(1) Embranchement des Cryptogams, and (2) Embranchement des Phanérogams. This demarcation of plants into two groups is not authorised by the present state of our knowledge and should be dropped. The definitions given of the sub-divisions are often inexact, as for instance in the case of Phanerogams (seed-plants would be a preferable term), which are defined as plants on which the highly differentiated sexual organs constitute a flower, and this, in spite of the fact that on p. 81 we are told that comparable associations of reproductive organs, to which it is difficult to refuse the name of flower, are found in other groups. The account of the Cryptogams occupies pp. 177-363, and all the rest of the book is given up to seed-plants. It is to be regretted that Prof. Courchet did not make him-

self acquainted with recent work in the Fungi, especially as regards the question of a sexual process; and a better and longer account should have been given of the Bryophyta. The arrangement of the seed-plants is practically that of Eicheer, but the treatment is not calculated to excite enthusiasm on the part of the student. It is full enough, in fact too full; the omission of the formidable-looking small print keys to the families, orders, and genera would have saved much space in the book and many groans on the student's part. A good feature is the description of a well-known plant in illustration of some of the natural orders; provided the plant is a typical one the student gets a concrete knowledge of the order of which it is a representative. Enough has, however, been said to give those interested a notion of the value of Prof. Courchet's hand-book, and also to suggest wherein improvements may be made should other editions be called for.

Faulty Geology

Failles et Géogenie d'après les Observations et Découvertes faites dans le Nivernais. By F. Lefort. [Publié par les soins de l'Institut Scientifique du Sacré-Cœur à Paray-le-Monial.] Pp. 259, with 6 pl. of sections, and one large map (1 m. 10 by 1 m. 20.) 4°. Paris: H. Le Soudier, 1897. Price, 20 fr.

"Geology is the study of the planet on which we live. Its special aim is the definition of the different mineral materials which compose the earth," begins M. Lefort. It would be discourteous to M. Lefort after this assurance of his belief to express any doubt as to whether this be his real idea of the special aim of geology. But if it be, then geology must have an extra special aim—the confusion of the evolutionists and the justification of Moses. M. Lefort's work is a bulky quarto, and it is illustrated by an enormous map of the Nivernais; but unfortunately neither of them can be regarded as a serious contribution to scientific progress; the map is cut up by a vast series of faults, which cross at regular angles, and make the map look like a mosaic of lozenge-shaped pieces of different colours. The faults are classified according to their exact orientation. It is more pretty than plausible. The Nivernais has been mapped by the Geological Survey of France, and most of the author's faults are omitted from the official maps. We must say that on comparison of the two, the latter appear by far the more plausible.

The text is not easy to summarise, but a series of quotations will illustrate the author's standard. There is a great deal of petrology in the volume, and its value can be estimated from the following: "The granites are more ancient than syenites and porphyries; after them come the trachytes and basalts, and finally the lavas of modern volcanoes." As an example of his palaeontology we may quote the following: "Les plus anciens crinoides sont supérieurs à leurs plus récents congénères; les échinides qui ont possédé 125 espèces n'en comptent pas aujourd'hui 50. Les poissons des premiers temps l'emportent sur ceux qui peuplent actuellement nos mers. Les labyrinthodontes, amphibiens qui vont apparaître tout d'un coup gigantesque pendant l'ére Triasique, ne sont pas moins supérieurs aux batraciens qu'ils ont devancés. On ne leur connait ni ancêtres ni descendants" (p. 16). The author also quotes with obvious approval an expression of surprise that a "célèbre professeur de géologie à l'Institut catho-

lique de Paris" should have written a history of the world, "sans citer une seule circonstance où Dieu est intervenu." This mistake the author of the present work certainly manages to avoid. "Il y a," he tells us, "des cataclysmes recurrents," possibly to be attributed to the Evil One; and he continues, "chacune de ces révolutions a anéanti les êtres vivants. La création d'une faune nouvelle a été nécessaire chaque fois ensuite, pour repeupler la terre."

The view that man has descended from some lower vertebrate is dismissed very emphatically, with language which is clearer than the reasoning: "L'existence de reptiles vertébrés munis de vertébres extrêmement rudimentaires, pendant que les os de leurs membres ont des épiphyses, et que les os craniens sont très développés, donne un démenti formel à la croyance que l'homme puisse dériver d'un animal vertébré."

The view of the antiquity of man is even more summarily dismissed: "L'idée d'un homme qui aurait vécu à l'époque Tertiaire est une hypothèse gratuite, qu'aucun savant de bonne foi n'a pu étayer d'un seul fait positif." Man dates only from the Quaternary, which is one of the three periods of the "Contemporary Epoch," of which the total duration "ne dépasse certainement pas 80 siècles."

From the above extracts our readers can form their own estimate of the value of the book. Possibly its main object was to inform the authorities of the "Institut Catholique de Paris" that, should they want a new professor of geology, it is quite possible to get men who can write big books about geology, and who are orthodox enough for cardinals.

A Guide to Zermatt

The Valley of Zermatt and the Matterhorn: A Guide. By Edward Whymper. 8vo, pp. xiv+212, with 2 Maps, Panorama, and 77 Illustrations. London: J. Murray, 1897. Price, 3s.

It is not often that guide-books have any literary merit, or any value beyond the immediate needs of tourists, except for topographic reference. Mr Whymper's guide to Zermatt is an exception. The illustrations are all of artistic merit; the text is not only full of exactly the information that a tourist wants, but it is written in perfect English. The book is full of touches of subtle humour, and tells such tersely and thrilling stories that it is as racy and interesting as a novel. The first chapter narrates the history of Zermatt before climbing days, and quotes the evidence to show that the Theodole Pass into Italy was used in Roman times. The first English visitor went to Zermatt in 1800, while English mountaineering began there with the ascent of the Strahlhorn in 1854 by the three brothers Smyth. The succeeding four chapters tell the story of the first attempts upon the Matterhorn, of the first successful ascent and its tragic end, and of the subsequent history and accidents. Then follows the information as to the routes to Zermatt, an account of the present village and its resources, and of the principal excursions. Much valuable information as to guides and their tariffs, lists of peaks and passes, and tables for the conversion of metres into feet, are given in the appendix. The work is a model of what a guide-book should be.

The Geology of Cheltenham

Cheltenham as a Holiday Resort. Part I. The Neighbouring Hill Country. By S. S. Buckman, F.G.S. 8vo, pp. vii. 100. Cheltenham, 1897.

This little work comprises a series of gossiping articles which were originally published in the *Cheltenham Examiner*, and which are descriptive of that charming region, the Cotteswold Hills. With this country Mr Buckman, and his father before him, have been associated for a long period, though not without interruption, for the parental home was transferred from Cheltenham to Cirencester, and finally into Dorset. Leaving this southern county, our present author has found successive homes at Andoversford, Stonehouse, and Charlton Kings, near Cheltenham, and he has not only made himself familiar with the scenery and geology of the breezy uplands and the picturesque combes, but has written some amusing sketches of the manners and customs of the rural inhabitants.

His present object is mainly to serve as a guide to those in search of the picturesque, although not overlooking the needs of those in search of scientific information. The work is therefore hardly open to scientific criticism, it is for the casual visitor rather than for the geological student—for the latter would find most of the detailed information he wants in Witchell's "Geology of Stroud" (1882), while Lycett's "Handbook to the Cotteswold Hills" (1857), though forty years old, will ever remain of great value and interest to the geologist.

The author's geological notes refer mainly to the Inferior Oolite, and he enumerates in his first section the several local subdivisions of the freestones and ragstones. He speaks of a "new school of geologists" who give names to the time during which the rocks were deposited—but surely Quenstedt and Oppel forty years and more ago recognised the need of chronological terms and used them, and no geologist has, so far as we are aware, disputed their value apart from and in addition to the local stratigraphical terms. The author would have done well to be a little more explicit in the use of such terms as "*Valdani* beds" and "*Striatus* beds," mentioned on the very first page, although he observes in his preface that "technical information has been purposely avoided." The term "*Valdani* beds" has rarely been used in this country. We may trust, however, that this little work will stimulate enquiry, as a great deal of useful information has been gathered together by the author on archaeology and topography as well as on his special subject of geology; and he notes here and there favoured spots suitable for "picnics," information which may be alike useful to the casual visitor as well as to the severer student of natural science.

A description of the Great Auk (*Alca impennis*) in the Natural History Museum at Amiens, France, has been published by H. Duchaussoy (Piteux, Amiens, 1897).

Serials

Barth, Leipzig, is now publisher of the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane.*

Timehri, the well-known publication of the Royal Agricultural Society of British Guiana, is discontinued with the December number.

An interesting series of papers on the Geology of the Congo Basin is appearing in *Le Mouvement Géographique*. They are compiled by Professor Jules Cornet.

We hear that, with the retirement of Mr H. C. Mercer and Prof. H. C. Warren from the board of associate editors of the *American Naturalist*, anthropology and psychology will no longer occupy the pages of that journal.

Mr G W. Murdoch, the able editor of the Natural History column in the *Yorkshire Weekly Post*, is preparing a guide to Lakeland, shortly to be published by Messrs Ward, Lock & Co. It will contain an exhaustive chapter on the zoology and botany of the district.

The Asa Gray Bulletin is a botanical journal, formerly the organ of a chapter of the Agassiz Association in the United States. With its June number it began to appeal to the general public in the shape of a bi-monthly magazine, published at Washington, D.C., price 50 cents. It is edited by G. H. Hicks with the help of A. J. Pieters, C. C. Dubois, and L. H. Dewey.

The *American Journal of Archaeology*, hitherto edited by Professor Forthingham, is now under the control of the Archaeological Institute of America, which will for the future issue its papers, reports, &c., in a regular and uniform publication. An editorial board, elected by the Council of the Institute, with Professor John H. Wright of Harvard as editor-in-chief, will conduct the new series of the journal. The Macmillan Company, New York, are the publishers.

With the December number of our lively and useful contemporary, *Nature Notes*, the editorship passes out of the hands of Mr James Britten, who has held it for six years, into those of Mr J. L. Otter, of 16 Vernon Terrace, Brighton. This journal has hitherto been remarkably successful in excluding faddists from its pages, and we trust the new management will be equally skilful in avoiding an obnoxious race who too frequently mar the pages of popular magazines.

L'Intermédiaire des Biologistes is the title of a new journal devoted to biology, and published in Paris. The first number appeared on 5th November 1897, and the second on the 20th of the same month. The aim of the Journal is to be a sort of *Notes and Queries* of zoology in all its branches in the first half of each number, and a review and list of new literature in the second half. Such an aim should find many supporters. *L'Intermédiare* will appear fortnightly; the subscription for the Postal Union is 12 francs; and the publisher is C. Reinwald, 15 rue des Saints-Pères, Paris. The Editors are Alfred Binet and Victor Henri.

Further Literature Received.

Recueil des Lois de la Biologie Générale, A. L. Herrera: Mexico. Nature and a Camera, R. Kearton: Cassell. Petrology for Students, ed. 2, A. Harker: Cambridge Press. Hand-book to the Geology of Cambridgeshire, F. R. C. Reed: Cambridge Press. The Argument of Adaptation or Natural Theology Reconsidered, G. Henslow: Stoneman. The Lepidoptera of the British Islands, vol. iv., C. G. Barrett: L. Reeve. Cape of Good Hope Meteorol. Comm., Rainfall 1885-94, A Buchan. Bericht über die Verwaltung und Vermehrung der Königl. Samml. f. Kunst u. Wissensch. Dresden, 1894-95. Museum of S. African Republic, List of Acquisitions, Oct. 1897. Johns Hopkins Univ. Circular, vol. xvii. no. 132. Proc. Roy. Soc. Victoria, vol. x. (n. s.) pt. i.

Earthquake of 12th June, R. D. Oldham: *Rec. Geol. Surv. India.* Fractional Crystallization of Rocks, and The Witwatersand Banket, G. F. Becker: *Amer. Journ. Sci.* and *18th Ann. Rep. U. S. Geol. Surv.* On *Spencerites*, D. H. Scott: *Abstr. Proc. Roy. Soc.* Phylogeny and Taxonomy of Angiosperms, C. E. Bessey: *Botan. Gaz.* Untersuchungen über die Fauna der Gewässer Böhmens, iii., A. Fritsch and V. Vavra: *Archiv. Naturw. Landesuntersuch. Böhmen.* On *Chlamydoselachus anguineus*, Garm., a remarkable shark found in Norway, 1896, R. Collett: *Univ. Festskrift Christiania.* Morphological and Biological Observations on South African Plants, also Nesting Habits of *Tockus melanoleucus*, S. Schönland: *Trans. S. African Phil. Soc.*

Amer. Journ. Sci., Dec.; l'Anthropologie, Sept.-Oct.; Botan. Gaz., Nov.; Feuille des Jeunes Nat., Dec.; Halifax Nat., Dec.; Irish Nat., Dec.; Journ. School Geogr., Nov.; Knowledge, Dec.; Literary Digest, Nov. 13, 20, 27, Dec. 4, 11; Naturae Novit., nos. 21, 22; Naturalist, Dec.; Nature, Nov. 25, Dec. 2, 9, 16; Nature Notes, Dec.; Naturen, Nov.; New Age, Oct.; Rivista Quind. Psichologia, vol. i. fasc. 12-15; Revue Scientifique, Nov. 20, 27, Dec. 4, 11; Science, Nov. 12, 19, 26, Dec. 3. Scientific American, Nov. 13, 20, 27, Dec. 4; Scot. Geogr. Mag., Dec.; Scot. Med. and Surg. Journ., Dec.; Victorian Nat., Nov.; Westminster Review, Dec.

OBITUARIES

RUDOLF P. HEIDENHAIN

BORN AT MARIENWERDER, JAN. 29, 1834. DIED OCTOBER 12, 1897

SON of a medical practitioner, Heidenhain was from the year 1859 until his death, professor of physiology and histology at the University of Breslau. He became celebrated principally through his work on the physiology of the muscles and of the nervous system, and on the functions of glands.

The chief of his important researches on the muscles were his *Mechanische Leistung, Wärmeentwicklung und Stoffumsatz bei der Muskelthätigkeit*, Leipzig, 1864; his original experiments on the tone of the muscles (1856), on muscular tetanus (1858), on pseudo-motor nervous actions (1883), and a remarkable series of studies on the innervation of vessels (1869-1878). To this series may also be added the memoir, written in collaboration with Bübnoff—*Ueber Erregungs- und Hemmungsnerven innerhalb der motorischen Hirncentren* (*Archiv. f. die ges. Physiol.*, 1881),—which propounds and develops, although hypothetically, one of the most important laws of nervous action, namely, that in a normal state every excitation of a nerve-centre gives rise to a process of opposite sensation, consequently tending to put an end to the motion previously provoked.

Amongst Heidenhain's works on the nervous system should be quoted his researches on hypnotism (1880), which are some of the best of physiological investigations of those complex phenomena.

But it is in the vast field of glandular functions that Heidenhain made his greatest discoveries. Most of his experimental results have become classical—the application of the anti-secretory action of atropine (discovered by Keuchel) to distinguish between the vaso-motor nerves of the glands and the true secretory nerves; the determination of the secretory nerve of the parotid (the glosso-pharyngeal); the influence of the nervous system on the pancreatic and bile secretions; the variations of pressure in the excretory passages of the glands; the functional differentiation between the glandular cells of the pyloric region and those at the bottom of the stomach; the formation of ferments in the cells of the pancreas; the function of various elements of the kidneys; absorption in the small intestine; and finally a series of conceptions of deep interest on ferment-producing substances and processes.

Whatever the importance of the above, there are two works which, by reason of their originality and suggestiveness, are superior to all the rest of Heidenhain's results in this direction. It is well known that he was the first to conceive the idea of studying the histological modifications of glands after a prolonged excitation of the

secretory nerves. Many valuable ideas on the physiology of glands resulted from his first experiments. Again, during the years 1889-1892, he was brought to consider lymph as a product of the secretion of the cells in the walls of the blood capillaries, and from this all our ideas on the formation and circulation of lymph underwent revision. The theory may be contested, but it has evoked many new facts.

The discovery of lymph-producing substances made in the course of these researches is not the least considerable. A new chapter of physiology was opened. Thus Heidenhain appears to us not only as a skilful and successful experimenter, but also as a clear-sighted intellect.

A view now generally held, that of the specific activity of cellular elements, resulted from Heidenhain's numerous researches on the functions of the glands. He contributed to the abandonment of purely physical explanations of the phenomena of secretion.

Almost all the works of Heidenhain were published in the *Archiv. f. die ges. Physiol.*, a few in the *Archiv. f. die mikros. Anatomie.* A large proportion were collected and published in four volumes at Leipzig from the year 1861 to the year 1868, under the title *Studien des physiol. Institutes zu Breslau.* Every physiologist has read and thought over his fine and complete study of glandular functions, published in Hermann's "Handbuch der Physiologie."

Great activity prevailed in Heidenhain's laboratory. Many well-known physiologists of the day studied under his direction. Amongst these may be mentioned Grützner, Hürthle and Röhmann.

[The above is largely taken from an article by Professor E. Gley in *L'Intermédiaire des Biologistes*, No. 2.]

OSCAR FRIEDRICH VON FRAAS

Born January 17, 1824. Died November 22, 1897

THE veteran director of the Royal Würtemberg Museum of Natural History in Stuttgart, who had been in failing health for some time, died on November 22. Dr von Fraas was a native of Lorch in Würtemberg, and at an early age entered the theological faculty of the University of Tübingen in preparation for the Church. While there he came under the influence of Quenstedt, who inspired him with zeal in the study of geology and palaeontology. In 1847 he proceeded to Paris for a year, where he pursued his studies at the School of Mines under d'Orbigny and Elie de Beaumont. At the conclusion of his university career in 1850 he was ordained, and became a pastor in his native country, devoting his leisure only to his favourite scientific pursuits. In 1854, however, he left the Church and was appointed Conservator of the minerals and fossils in the Würtemberg Museum, an office which he held until a few years ago, when he succeeded Dr von Krauss as Director of the Museum. The researches of Dr von Fraas had reference chiefly to the geology, palaeontology, and prehistoric archaeology of Würtemberg; and in the rich collections of the museum which he did so much to form, he has left an enduring monument and memorial of his labours. But he also carried on important researches in the East, where he was one of

the pioneers in working out the geology. He made two journeys through Syria, the first in 1864-65, the second in 1875, and devoted special attention to the Lebanon. He described the results in a valuable little work entitled "Aus dem Orient." He was also much occupied with other researches beyond his special domain, and devoted special attention to the successful cultivation of the vine in Würtemberg.

We regret to learn of the death on October 21, owing to an accident on the lake, of JOHANNES FRENZEL, formerly professor of zoology at Cordoba University in the Argentine Republic, and of late years director of the biological and fishery station on the Müggelsee near Berlin. An account of the work here carried on so successfully by Dr Frenzel, who was only thirty-nine years old, was published in *Natural Science* for March 1897 by Professor Anton Fritsch (vol. x. p. 170).

DR JUSTIN WINSOR, librarian of Harvard University and also an energetic anthropologist and ethnologist, died on October 21, 1897, aged 66. He was a great authority on the early history and cartography of America.

The following deaths are also announced:—Dr HARRISON ALLEN, emeritus professor of comparative anatomy in the University of Pennsylvania, and well-known as a writer on mammals, at Philadelphia on Nov. 14; E. P. FRANZ, nerve-anatomist, and assistant to Professor E. A. Schäfer at University College, London; Dr MARK OLIVET, professor of psychiatry in the University of Geneva, and author of several medical and hygienic works, at Genoa on Oct. 24, aged 75; JAMES MURRAY, professor of pathology in the Lahore Medical College, on Nov. 8 at Simla, aged 32; ALBERTO PERUGIA, ichthyologist, at Genoa on Sept. 24, aged 54; ALEXANDER SOLON, student at the Brussels University, in Banana at the mouth of the Congo, in Oct., aged 22; Dr FRANKLIN STORY CONANT, student of chaetognathous worms, at Boston, on Sept. 13, in consequence of an illness contracted during the Johns Hopkins University Expedition to Jamaica in the summer of 1897; Dr WILHELM MOERICKE, privat-docent in mineralogy, at Freiburg i. B., on Oct. 8; JAMES WINDOES, a well-known collector of fossils, at Chipping Norton, aged 58; Dr GEORGE H. HORN, the eminent entomologist, one of the secretaries of the Philosophical Society, and formerly corresponding Secretary of the Academy of Natural Sciences, at Philadelphia, on Nov. 25, aged 58; GUSTAV STANDFUSS, lepidopterist, on Oct. 6, aged 82; FRANCISQUE GUILLEBEAU, entomologist, author of the catalogue of the Coeleoptera of Ain, at Plantay, aged 77; Dr G. LICOCOPOLI, assistant professor of botany at the University of Naples; HORACE W. L. BILLINGTON, director of botanical gardens, Royal Niger Coast Protectorate, on Nov. 18, aged 28; KARL MUELLER, director of the Agricultural Experimental Station in Hildesheim, and agricultural chemist, on Oct. 26, aged 50; FRIEDRICH STOHMANN, professor of agricultural physiology and agricultural chemistry in the University of Leipzig, on Nov. 1, aged 65; Dr BERNARDIN A. MARTIN, a zealous student of the flora of the Cevennes, on March 31, at Aumessan (Gard); and Dr ALBRECHT SCHRAUF, the mineralogist of Vienna, aged 59, in the middle of December.

NEWS

THE following appointments are announced :—

Dr J. Joly, to be professor of geology in Trinity College, Dublin ; Dr Wm. G. Smith, to be assistant-lecturer in botany, at the Yorkshire College, Leeds ; Dr J. D. E. Schmeltz, keeper at the Rijks Museum of ethnography in Leiden, to be director in the place of Dr L. Serrurier, resigned ; Dr Otto Fuhrmann, to be professor of zoology at the Neuchatel Academy ; Dr Max Dessoir, to be associate-professor of psychology in Berlin University ; Dr George Kraus, to succeed J. von Sachs as professor of botany at Halle ; Franz Mattouschek, assistant in botany at the German University in Prague, to be professor at the gymnasium in Linz ; Dr A. Ostroumoff, to be professor of zoology in the University of Kasan ; Dr Polumordwinow, to be privat-docent of histology and prosector in the University of Kasan ; Dr Alexis E. Smirnov, to be professor of zoology in the University of Tomsk ; Dr Guido Schneider, to be director of the Biological Institute at Sebastopol ; Clarence L. Herrick, to be president of the University of New Mexico in Albuquerque ; Dr Geo. T. Kemp, to be professor of physiology in the University of Illinois ; Dr John Y. Graham, of Princeton, to be professor of biology in the University of Alabama ; Robert B. Yound, to be assistant-biologist in the Department of Agriculture, at Washington ; A. A. Tylor, to be instructor in biology in the Union College, Schenectady, New York State ; Dr Frederick D. Lambert, to be assistant in biology at Tufts College ; Dr Lafayette B. Mendel, to be assistant-professor of physiological chemistry in Yale University ; T. A. Reakard, to be State geologist of Colorado ; Dr Schmitz-Dumond, of Tarand, to be director of the Agricultural Experimental Station to be established in Pretoria ; Dr Philippi, to be director of the National Museum in Santiago, Chili, in place of his father, resigned.

A CHAIR of geography has been established in Würzburg University.

THE seventh International Congress of Geography will be held in Berlin in the year 1899.

MR MARTIN FOUNTAIN WOODWARD will succeed Mr E. R. Sykes as Secretary to the Malacological Society.

MR SCHAAFFHAUSEN'S valuable anthropological collection has been left to the Museum of Bonn University.

DR RICHARD SEMON has resigned his office as prosector and professor of anatomy in the University of Jena.

MANY fish were brought alive to the New York Aquarium, but the attempt to transport invertebrates was unsuccessful.

DR SVEN HEDIN, the Swedish explorer of Central Asia, is the recipient of the Danish Geographical Society's gold medal.

PROFESSOR SCHÄFER of University College, London, is the recipient of the Baly Medal of the Royal College of Physicians.

THE Senckenbergische Naturforschende Gesellschaft of Frankfurt has purchased the library of the late Professor Carl Vogt.

THE Durham College of Science, Newcastle-on-Tyne, has, on October 21, established a marine biological station at Cullercoats (Northumberland).

THE Hallett Philips collection of Indian implements and antiquities from the Potomac Valley has, says *Science*, become the property of the Smithsonian Institution.

THE University of Lyons is, says *La Feuille des Jeunes Naturalistes*, devoting a sum of 42,000 fr. to the completion of the biological laboratory of Tamaris, near Toulon.

THE general plans of the Zoological Gardens in Bronx Park, New York, were approved without opposition by the New York Park Board at the public hearing on November 22.

PER DUSÉN, the Swedish engineer and bryologist, has returned from Tierra del Fuego and Patagonia, where he has been engaged in scientific research since September 1895.

THE John Lucas Walker Studentship for Research in Pathology at Cambridge has been awarded to Mr J. W. Stephens, B.A., of Caius College, and Dr Hamilton Wright of Montreal gains the Exhibition of £50.

THE chair of psychiatry will shortly be vacant in the University of Zürich, owing to the resignation of Dr August Forel, who is also well known as a student of the habits of insects, especially of ants.

MR W. P. PYCRAFT has left Oxford, and has been appointed temporary assistant in ornithology in the British Museum (Natural History). He will devote his attention specially to the arrangement of the collection of skeletons of birds.

THE Danish Government is organising for 1898 a geographical and ethnographical expedition to the Pamirs, where two years will be spent in research under the direction of Lieut. Olofsen. Two specialists will accompany the expedition.

MR J. B. HATCHER, we learn from *Science*, has returned to Patagonia to make further investigations in geology and palaeontology. He hopes to add natural history and ethnological collections to the palaeontological collections already made in his recent successful expedition to that country.

BY the will of the late Sir William Alexander Mackinnon the University of Edinburgh receives £2000 upon trust to found prizes or scholarships to be named the "Mackinnon Scholarships," for proficiency in geology, natural history, and modern languages. Bequests are also made to the Royal Society of London.

ON July 29th a Museum of Natural History and Anthropology was opened at Wernigerode, in Brunswick. The museum, says *Science*, comprises the following collections:—Mineralogical, of Count Heinrich Ernst, Dr Döring and Councillor Jasche; zoological, of Dr Müller; anthropological, of Dr Augustin and Dr Friedrich; and the Herbarium of Dr Sporleder.

DR JOSEPH GEDGE, who died in 1870 while with Sir Samuel Baker at Khartoum, left a sum of £1000 to the University of Cambridge for the establishment of a biennial prize for original physiological research, open only to graduates of more than five and less than seven years' standing. Owing to a long delay this bequest has only lately been received by the University.

THE Boston *Transcript*, as quoted by *Science*, states that owing to the disastrous effects of the yellow fever epidemic in the South, the American Public Health Association appointed a committee of seven to urge on President M'Kinley the absolute necessity of a special commission of experts, to be appointed by Congress, to make a thorough study in Havana of the cause and prevention of the disease.

AT the Annual Meeting of the Scarborough Field Naturalists' Society last month, a most gratifying report of the past year's local scientific work was read. The Society is fortunate in possessing at least one member to undertake systematic observations in nearly every group of the local fauna and flora, and records are carefully kept.

A SHOCK of earthquake was felt at Kudat in British North Borneo, as also a slight tremor at several places along the coast on September 21. About the same time a new island was thrown up from the sea between Lambeidan and Mempakul, opposite Labuan, and about 50 yards from the mainland. The island emits inflammable gas, with a strong smell of petroleum, and appears to be increasing in size.

THE Regents of the University of the State of New York have, says *Science*, authorised relief maps of the Adirondacks and another of Manhattan Island, showing its physical features before alteration by civilisation. They have also issued a notice to University institutions that duplicates of these maps will be furnished to the schools for cash or as a part of their apportionment, at a trifling cost.

WE understand that Colonel Bernier will leave England on March 1, in Mr Harmsworth's ship, the *Windward*, in an attempt to reach the North Pole. According to his present programme, he will first proceed to the north coast of Siberia, and thence will journey towards the Pole over the ice. Assuming that he will move along at the rate of six miles a day, he estimates that he will reach it in 120 days. He will then return to Spitzbergen.

THE Pirogof Museum of Surgery and Anatomy, which was opened in St Petersburg at the beginning of December, will not only serve as a museum but also as a place of meeting for all the medical societies of that city. The cost of the building deducted, there remains a margin for endowment from the £12,000 devoted to it, half of which was bequeathed by Mrs Musin-Pushkin, the remainder being in the form of subscriptions.

THE Biological Department of New York University selected the Bermudas as the ground for its first expedition, under the direction of Dr C. L. Bristol, who has given an account in *Science*. Harrington House, about six miles from Hamilton, was made headquarters, and a laboratory installed near Castle Harbour. It is proposed to establish a station in these islands, and to make them the centre for further exploration in the West Indies. Dr W. M. Rankin of Princeton will report later on the collections made.

WASHINGTON will henceforth be the headquarters from which will be carried on the tropical work of the Sub-tropical Laboratory of Eustis, Florida, under the direction of the Division of Vegetable Physiology and Pathology of the Department of Agriculture. Work can be done and experiments made at Miami, Florida, where a small garden tract and a laboratory have been secured. Experiments have been made in hybridising the orange and other citrous fruits, and the resulting hybrids will be cultivated on this land. It is also proposed to introduce and test varieties of tropical plants which can be successfully grown in the tropical and sub-tropical portions of the United States.

THE new regulations for the Intermediate Science Examination of London University (which are destined to come into force in 1899) allow candidates to offer any three of the following four subjects :—(*a*) Mathematics, (*b*) Chemistry, (*c*) Physics, (*d*) Botany *plus* Zoology. A meeting of teachers of botany and zoology engaged in training students for the examinations of London University, thinking this arrangement prejudicial to the interests of the biological sciences, thus held a meeting on November 30, 1897, to discuss the subject. The meeting was of opinion that (1) zoology and botany being separate branches of knowledge, taught in separate laboratories by separate teachers, and tested by separate papers in the examination, should be treated as independent examination subjects ; (2) that the necessary result of the new regulations would be, that students, on finding that to offer botany and zoology would entail upon them the study of four subjects instead

of three, would be unfairly diverted from these two sciences to the three which entailed less examination work. The standard of knowledge required for the examination in botany and zoology has been in both cases admittedly as high as that demanded in each of the other subjects, and will doubtless be maintained by the examiners. A memorial embodying these views has thus been presented to the University Senate.

We have frequently noticed the admirable publications of the Institute of Jamaica, and we have now received its Annual Report for the year ending March 31, 1897, which is a record of great and successful activity. During the year changes have been made in the arrangement of the Museum, and numerous important additions are recorded. In the Department of Zoology many birds' nests have been mounted and displayed with the specimens of birds. The latter have been re-labelled. All the fishes in alcohol and formalin have been mounted on glass, re-labelled, and several additions have been made. The Decapod Crustacea have been named and arranged on tablets. Many of the Echinodermata, Actinaria, and Corals have been provisionally named and displayed. A four to six per cent. solution of formalin has been mostly employed as a preservative fluid and found to work very satisfactorily, though the colours of the objects are not retained for long in specimens exposed to strong light. For delicate objects such as jelly-fishes and sea-anemones, it is found to be extremely serviceable, perserving perfectly the natural form and histology. In the Department of Botany additions have been made to the Herbarium by the Hon. Wm. Fawcett, Director of Public Gardens and Plantations. In the Department of Geology an extensive students' collection of fossils has been purchased and arranged, in two cases with descriptive labels. The series illustrates by actual fossils and casts the typical forms of life met with throughout the geological formations. The mineralogical collection has been grouped so as to illustrate, in an elementary manner, the different branches of interest in the subject. Mr F. C. Nicholas has presented an elementary series of fossils, with illustrative recent shells, and these have been biologically arranged. Some of the specimens from the Survey Geological collections have been lent to Professor R. T. Hill of the American Geological Survey, who is at present engaged upon a study of the Geology of Jamaica, and has already carried out two explorations. Various collecting expeditions have also been made by the Curator, Mr J. E. Duerden, with very satisfactory results.

Our American contemporary *Puck* prints the following jest regarding the numerous attempts to reach the North Pole :—

The North Pole Calendar

(*For the next Nine Years*).

1898.—Expedition of Haans Haansen, from Haamerfest, Norway, to find the North Pole.
1899.—Expedition of Torm Tormpsen to find Haansen.
1900.—Expedition of Jhim Jhornsen to find Tormpsen.
1901.—Expedition of Jjaik Jjaksen to find Jhornsen.
1902.—Return of Jjaksen with relics of Jhornsen. Marble monument erected to the memory of Jhornsen.
1903.—Return of Jhornsen with relics of Tormpsen. Brick monument erected to Tormpsen.
1904.—Return of Tormpsen with relics of Haansen. Wooden monument erected to Haansen.
1905.—Return of Haansen, dead-tired. North Pole still standing.
1906.—North Pole discovered by an American. Auction sale of monuments at Haamerfest.

CORRESPONDENCE

WASPS

Mr O. H. Latter (*Natural Science*, vol. x., p. 368-9) invites information on the subject of the effect of climatic influences on wasps, and as the past summer was certainly a remarkable one in this respect, perhaps the following observations may be worth publishing:—

My experience here agrees with Mr Latter's to some extent, but not entirely. Queens were fairly abundant in May, but from the time the last queen disappeared till the beginning of September I don't think I saw two workers except from a single nest of *Vespa norvegica*, which was taken in a hedge. But in my diary on September 4, I have the following entry:—"Wasps, which have been extraordinarily scarce this year, have suddenly become very abundant in the house, though seldom seen outside." This was during a spell of unusually cold weather for early September.

As to the climatic causes which produced the summer scarcity, it is not very obvious how the wet of the winter of 1896-7 could have affected the hibernating queens, as Mr Latter suggests, because he says they were abundant in spring; moreover, they generally hibernate in places where they are safe from rain. It is generally supposed that a cold spring is destructive to the queens, with the natural consequence of a scarcity of workers in the summer. But Mr R. Newstead, F.E.S., who has paid much attention to wasps, writes me that he finds in his notes that "in 1889 wasps were exceptionally abundant in Denbighshire and Cheshire." It will be seen from the annexed table of temperatures (which are from corrected instruments in a Stevenson screen in my garden) and rainfall, that the spring of 1889 was on the whole rather colder and very much wetter than that of 1897, April in particular being the wettest I have recorded in eighteen years.

	Mean Temperature.			Lowest in Shade.			Rainfall.		
	Mar.	April.	May.	Mar.	April.	May.	Mar.	April.	May.
	Degs.	Degs.	Degs.	Degs.	Degs.	Degs.	In.	In.	In.
1889 . . .	41·5	43·9	53·1	23·6	32·3	36·3	1·90	4·29	2·91
1893 . . .	47·3	51·3	56·0	31·5	30·0	39·8	0·36	0·62	1·38
1897 . . .	45·0	45 0	50·1	34·3	31·8	36·0	3·22	2·53	0·99
Average of 17 years	41·1	46·4	51·4	—	—	—	2·06	1·68	2·05

It would seem, therefore, that climatic influences do not altogether account for the abundance or scarcity of wasps, still less for their late appearance this year. A satisfactory explanation is still wanted.

Alfred O. Walker.

Nant y Glyn, Colwyn Bay.

NOTICE

To Contributors.—All Communications to be addressed to the Editor of Natural Science, at 29 Bedford Street, London, W.C. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

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